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ESSAYS,

ON THE

EFFECTS PRODUCED BY VARIOUS PROCESSES ON

ATMOSPHERIC AIR;

WITH A PARTICULAR VIEW

TO AN INVESTIGATION OF THE

CONSTITUTION OF THE ACIDS.

By M. LAVOISIER,
Member of the Royal Academy of Sciences at Paris, &c.

TRANSLATED FROM THE FRENCH,

By THOMAS HENRY, F. R. S.
Member of the *Medical Society* of London, and of the *Literary and Philoſophical Society* of Manchester.

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THE TRANSLATOR's

PREFACE.

IN a volume of Eſſays Phyſical and Chemical, written by M. Lavoiſier, which I tranſlated and publiſhed a few years ſince, a number of additional eſſays were announced, on various important ſubjects, with which that ingenious philoſopher promiſed ſoon to favour the public. Other avocations, it ſhould ſeem, have prevented the fulfilling of his engagement, and he appears to have been principally occupied in an attempt to overthrow Stalh's doctrine of phlogiſton, and in an inveſtigation of the nature and conſtitution of the acids. The reſult of his inquiries he has communicated to the royal academy of ſciences in a ſeries of memoirs, a tranſlation of which is now preſented to the public.

Phlogiston had been ſo much talked of, though never ſeen, by chemiſts, being cogniſable only by its effects, that many began even to doubt its exiſtence, and to regard it rather as a creature of the imagination, than as a real and powerful agent, concerned in ſome of the moſt important operations of nature. Among others, our author had entertained theſe doubts; and, imagining he ſaw ſtrong objections to Dr. Prieſtley's theory of the phlogiſtication of air, he thought the changes effected in it, by various proceſſes, might be accounted for on different principles.

Dr. Priestley had ſuppoſed that fixed air is one of the conſtituent parts of common air, and that, whenever a quantity of phlogiſton is thrown into the latter, it ſeparates the fixed air, and becomes united to the remainder, which has a greater affinity to it than to the fixed air, and forms phlogiſticated air.

M. LAVOISIER however contends that this is not the caſe; that atmoſpheric air conſiſts of about ¼ of very pure air, and ¾ of mephitic gas, which, when detached from its purer portion, is no longer fit for the purpoſes of air; and he denies that any fixed air is naturally contained in atmoſpheric air, but aſſerts that it proceeds from the union of coaly matter with the above pure part or Dr. Prieſtley's dephlogiſticated air, a term which M. Lavoiſier is not altogether diſpoſed to admit.

ANOTHER objection of M. Lavoiſier to Dr. Prieſtley's theory of the phlogiſtication of air, was founded on the different views in which theſe two philoſophers beheld the phænomena which take place during the calcination of metals, and their ſubſequent reduction to a metallic form. Dr. Prieſtley had ſuppoſed that a ſeparation of the fixed air being effected by the phlogiſton, this air is united to the metallic calx, and that, during the reduction, the calx recovers its phlogiſton, and parts with the fixed air. On this M. Lavoiſier obſerves, that it is probable the air abſorbed is not fixed, but pure air, and that, could we reduce the calxes without addition, it would be ſeparated again pure. This is actually the caſe with the calx of mercury, which requires no addition for its reduction, and yields pure dephlogiſticated air when it recovers its metallic form. But the other calxes requiring the addition of ſome coaly matter, this, he imagines, debaſes the pure air, and changes it into fixed air. The agency of phlogiſton, therefore, in this caſe alſo, he regards as gratuitous, and not ſufficiently founded on facts.

THE exiſtence of phlogiſton, however, has not only been proved, but Dr. Prieſtley has clearly ſhewn that phlogiſton and inflammable air are the ſame thing, with this difference only, as Mr. Kirwan remarks, that when, in a ſtate of elaſticity, it poſſeſſes ſo much fire as is requiſite to give it the form of vapour, and that this air is capable of being wholly abſorbed in the reduction of metals, and of reſtoring to the calxes their priſtine metallic ſplendor and malleability.*

THERE can be little doubt, therefore, that the phlogiſtication of air really takes place in many proceſſes; but perhaps it may produce its effects in a manner ſomewhat different to what Dr. Prieſtley originally imagined. The idea of that excellent philoſopher ſeemed at firſt well founded; but recent facts have thrown a different appearance on the ſubject. Of theſe none are more convincing than the experiments of M. Lavoiſier on the combuſtion of pyrophorus, by which almoſt the whole of the dephlogiſticated air employed, inſtead of being phlogiſticated in the ſenſe of Dr. Prieſtley's term, was converted into fixed air.

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* I AM informed that Dr. Prieſtley has delivered an account of his intereſting experiments and remarks, on this ſubject, to the Royal Society.

Now as the being of phlogiſton can be no longer doubted; as Dr. Prieſtley has literally given "*to airy nothing a local habitation and a name*;" and has embodied and rendered viſible this Proteus which has ſo long eluded the graſp and ſight of the chemiſt; the truth may, perhaps, without much difficulty, be found, by changing M. Lavoiſier's term coaly matter, *mattière charbonneuſe*, for that of phlogiſton; and allowing that, inſtead of wholly uniting with the reſiduum, it partly combines with the pure or dephlogiſticated portion of the common air, and thereby forms the aërial acid or fixed air, which is, in fact, a true phlogiſticated acid.

THE other objection has been anſwered by Mr. Kirwan, in his admirable paper on the ſpecific gravities, &c. of ſaline ſubſtances.* Mercury affords inflammable air, and conſequently contains phlogiſton. This phlogiſton muſt fly off during the calcination, and form fixed air, which will be abſorbed by the calx. And though the calx, when reviving, yields dephlogiſticated air, this may depend upon the mercury's attracting the phlogiſton from the air, and applying it to its own reduction.

THAT fixed air contains phlogiſton has been proved by Mr. Kirwan, and he has even determined the amount of it to be in the proportion of 14,661 grains of phlogiſton, and 85,339 of pure air in 100 grains of that gas. And he computes that 100 cubic inches of dephlogiſticated air are converted into fixed air by 7,2165 grains of phlogiſton, and will then be reduced to the bulk of 86,34 cubic inches; and that, reciprocally, 100 cubic inches of fixed air being decompoſed, will afford 115,821 cubic inches of dephlogiſticated air, and part with 7,2165 grains of phlogiſton, ſuppoſing the decompoſition to be complete, or, in other words, the dephlogiſticated air to be quite pure.*

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* Phil. Tranſ. Vol. LXXII. p. 227.

We therefore ſeem to be arrived at a pretty perfect knowledge of the conſtituent parts of one of the acids, viz. the aërial, which appears to be compounded of the pure part of atmoſpheric air, united with phlogiſton, and, in its elaſtic ſtate, alſo containing a portion of latent heat, or as M. Lavoiſier, perhaps as properly calls it, fixed fire.

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* Phil. Tranſ. Vol. LXXII. p. 236.—Mr. Kirwan alſo mentions Dr. Prieſtley's very candid approbation of his account of the conſtitution of fixed air, though ſo contrary to opinions which the Doctor had advanced in his laſt publication. An inſtance of philoſophical candour highly to be admired and worthy of imitation.

Many of the enſuing eſſays ſtrongly tend to demonſtrate that this pure air enters, alſo, as a principal component part, into the conſtitution of the other acids;* but we yet remain unacquainted with the true nature of the other portion, by the variety of which, each acid ſeems to be diſtinguiſhable from the others. M. Lavoiſier has ſuppoſed the vitriolic acid to be formed by the junction of ſulphur to pure air, and that ſulphur is formed by depriving vitriolic acid of this air. Should we not rather ſay, that when vitriolic acid is formed, the

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[*] The Abbè Fontana and Mr. Berthollet had before ſhewn, that the acid of ants and the vegetable acids conſiſt of fixed air, or are, at leaſt, reſolvable into it. Vide Cavallo's Treatiſe on Air, p. 607.

ſulphur throws off its phlogiſton, a part of which, joining with the pure portion of the atmoſphere, is again attracted by the baſis of the ſulphur, and, thus combined, forms the acid; and, on the contrary, that when this acid is reconverted into ſulphur, the baſis parts with the pure air, and recovers its phlogiſton?

An experiment of Dr. Prieſtley's, which he lately communicated to Dr. Percival, greatly elucidates and almoſt confirms this opinion. Having mentioned his having formed ſulphur by the union of inflammable air with vitriolic acid, as a proof of the identity of that air with phlogiſton; he adds, that he had alſo proved, more unexceptionably than before, that the electric matter contains phlogiſton, by making it to paſs through the air, confined by the acids, in a ſyphon. "When," ſays he, "I uſe the "dephlogiſticated marine acid, the air is "diminiſhed by the proceſs, and dephlo-

" giſticated. If I uſe the *phoſphoric acid* " or the *phlogiſticated alkali*, the air is firſt " diminiſhed, and then increaſed by an " addition of inflammable air. If I uſe " VITRIOLIC ACID *or the* NITROUS ACID, " *there is a production of* DEPHLOGISTICATED AIR, *faſter than the electri*" *city can injure it*." Now, from whence can this dephlogiſticated air proceed, but from a decompoſition of the acids by the phlogiſton of the electric matter, in which the pure air is ſeparated, while the phlogiſton, combining with the remaining part or baſis of this acid, forms a ſulphur?

WHETHER the phlogiſton, by means of which the pure air is ſeparated, and which uniting therewith forms fixed air, be admitted into the compoſition of the acid, or rejected when that air loſes its elaſticity, is not as yet aſcertained. It is probable, that the acid not only receives its acidifying principle from the pure air, but alſo its fire or abſolute heat. Now though Dr. Crawford has ſhewn that bodies which contain much phlogiſton poſſeſs little abſolute fire (theſe ſubſtances repelling each other; ſo that the addition of phlogiſton to any ſubſtance expels from it a part of its fire, and, on the other hand, the addition of fire ſeparates a part of its phlogiſton) yet it appears that their coexiſtence is not incompatible; inflammable air, as was before obſerved, owing its elaſticity to a portion of fire with which it is combined.* Now as a ſmall portion of phlogiſton, added to vitriolic acid, makes volatile vitriolic acid, and a larger doſe fixes it and forms ſulphur, perhaps a very minute quantity may be neceſſary to the compoſition of the acid.

M. Lavoisier objecting, and with ſome reaſon, to the appellation of fixed air, has, in the ſubſequent eſſays, denominated it, the *chalky aëriform fluid or acid*; and the academy not ſatisfied with this name, as being too confined, have preferred that of *gaſeous air*. But, as the cuſtomary term is beſt known to Engliſh readers, I have thought proper to retain the uſe of it, till philoſophers, in general, have agreed upon the adoption of a new one.

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* Kirwan, Phil. Tranſ. Vol. LXXII. p. 209.

THE

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ESSAY I.

EXPERIMENTS ON THE RESPIRATION OF ANIMALS, AND ON THE CHANGES EFFECTED ON THE AIR IN PASSING THROUGH THEIR LUNGS.

OF all the phenomena of the animal œconomy, none is more ſtriking, none more worthy the attention of philoſophers and phyſiologiſts than thoſe which accompany reſpiration. Little as our acquaintance is with the object of this ſingular function, we are ſatisfied that it is eſſential to life, and that it cannot be ſuſpended for any time, without expoſing the animal to the danger of immediate death.

It is univerſally known that air is the agent, or rather the ſubject of reſpiration; but at the ſame time all ſorts of air, or more generally ſpeaking, all ſorts of elaſtic fluids, are not proper for the purpoſe, nay, there are many kinds which animals cannot breathe, without periſhing, at leaſt, as ſoon, as if they had no air to reſpire.

The experiments of ſome philoſophers, and eſpecially thoſe of Meſſrs. Hales and Cigna, had begun to afford ſome light on this important object; and, Dr. Prieſtley has lately publiſhed a treatiſe, in which he has greatly extended the bounds of our knowledge; and has endeavoured to prove, by a number of very ingenious, delicate, and novel experiments, that the reſpiration of animals has the property of phlogiſticating air, in a ſimilar manner to what is effected by the calcination of metals and many other chemical proceſſes; and that the air ceaſes not to be reſpirable, till the inſtant when it becomes ſurcharged, or at leaſt ſaturated, with phlogiſton.

However probable the theory of this celebrated philoſopher may, at firſt ſight, appear;

however numerous and well conducted may be the experiments by which he endeavours to ſupport it, I muſt confeſs I have found it ſo contradictory to a great number of phenomena, that I could not but entertain ſome doubts of it. I have accordingly proceeded on a different plan, and have found myſelf led irreſiſtibly, by the conſequences of my experiments, to very different concluſions. It will not be neceſſary, at preſent, to diſcuſs, particularly, each of Dr. Prieſtley's experiments, nor to ſhew how they all contribute to confirm the opinion which I am proceeding to communicate in this memoir: it will be ſufficient to relate my own, and to give an account of their reſults.

In a convenient apparatus, which it would be difficult to deſcribe without the aid of engravings, fifty cubic inches of common air were incloſed, to which I introduced four ounces of very pure mercury, which I proceeded to calcine by keeping it, during twelve days, in a degree of heat almoſt equal to that which is neceſſary to make it boil.

Nothing remarkable occurred during the firſt day: the mercury, though it did not boil, was in a continual ſtate of evaporation, ſmall drops of it, which were at firſt very minute, lined the inſide of the veſſels, and gradually increaſing till they acquired a certain bulk, fell again by their own gravity to the bottom. On the ſecond day I could obſerve ſome ſmall red particles ſwimming on the ſurface of the mercury, which, in a few days, increaſed both in number and bulk. On the twelfth day, having extinguiſhed the fire and ſuffered the veſſels to cool, I obſerved that the air, which they contained, was diminiſhed to the amount of from eight to nine inches, viz. about $\frac{1}{6}$ of its volume: at the ſame time a conſiderable portion of mercurius precipitatus per ſe, or calcined mercury, was formed, which I computed to be about forty-five grains.

This air, which had been thus diminiſhed, did not precipitate lime-water; but it extinguiſhed candles, and animals, immerſed in it, periſhed in a ſhort time: it no longer afforded red vapours, when mixed with nitrous air, nor was diminiſhed by it. In ſhort, it was abſolutely reduced to a mephitic ſtate.

It has been proved by Dr. Prieſtley's, and alſo by my own, experiments, that calcined mercury is merely a combination of that metal with about $\frac{1}{12}$ part of its weight of air, much better and more reſpirable, if the expreſſion may be allowed, than common air: it ſhould appear then, as proved, that, in the preceding experiment, the mercury, as it calcined, had abſorbed the beſt and moſt reſpirable part of the air, and left only the mephitic or unreſpirable part. The following experiment confirmed me more and more in the truth of this opinion.

I carefully collected the forty-five grains of calcined mercury which had been formed in the preceding experiment; and putting it into a very ſmall glaſs retort, the neck of which was turned up ſo as to paſs under the edge of a bell-glaſs, filled with, and inverted into, water, I proceeded to reduce it without addition. By this operation I recovered nearly the ſame quantity of air which had been abſorbed during the calcination; namely, between eight and nine cubic inches, which, when recombined with the air which had been vitiated by that proceſs, reſtored the latter, pretty exactly, to the ſame ſtate in which it had been, previous to the calcination being performed in it, viz. that of common air; for now candles were not extinguiſhed in it; animals no longer periſhed in it, and it was nearly as much diminiſhed as atmoſpheric air, by the addition of nitrous air.

We have here the moſt complete proof, that chemiſtry can afford, of the decompoſition and recompoſition of air; from whence it evidently reſults, 1ſt. that $\frac{5}{6}$ of the air which we breathe, are mephitic, or incapable of ſupporting the reſpiration of animals, or the inflammation and combuſtion of bodies: 2dly. that the ſurplus, or $\frac{1}{5}$ only of the volume of atmoſpheric air, is reſpirable: 3dly. that, in the calcination of mercury, this metallic ſubſtance abſorbs the ſalubrious part, leaving only the mephitic portion of the air: 4thly. that by reuniting theſe two portions which had been ſeparated, we can recompound air, ſimilar to that of the atmoſphere.

These preliminary truths, relative to the calcination of metals, tend to lead us to plain conſequences concerning the reſpiration of animals; and as the air, which has ſerved ſome time for this vital office, has much relation to that in which metals have been calcined, our knowledge, relative to the one, leads us naturally to an application of it to the other.

I PLACED a ſparrow under a glaſs receiver, filled with common air and ſtanding in mercury, capable of containing thirty-one cubic inches: the bird did not ſeem at all affected at firſt, except that it was a little ſtupefied: in a quarter of an hour it began to be agitated; its reſpiration became laborious and rapid; and, from this inſtant, the ſymptoms of diſtreſs increaſed, till, at the end of fifty-five minutes, it died convulſed.—Notwithſtanding the heat of the animal, which neceſſarily, at firſt, rarefied the air in the receiver, there was a ſenſible diminution of its bulk, which at the end of fifteen minutes amounted to about $\frac{1}{40}$; but inſtead of increaſing afterwards, the diminution became ſomething leſs in about half an hour; and when the animal was dead, and the air in the receiver had recovered the temperature of the room where the experiment was made, the diminution did not appear to be more than $\frac{1}{60}$.

THIS air, which had been reſpired by the ſparrow, was become very different from that of the atmoſphere: it precipitated lime-water; extinguiſhed candles, and ſuffered no diminution by the teſt of nitrous air: another bird, introduced into it, exiſted but a few moments: it was, in ſhort, entirely mephitic, and, in that reſpect, appeared much ſimilar to that in which mercury had been calcined.

BUT a more attentive examination diſcovered to me two very remarkable differences between the two airs: 1ſt. the diminution of that, in which the ſparrow had died, was much leſs than that of the air which had been employed for the calcination; and, 2dly. the reſpired air precipitated lime-water, on which the other produced no change.

THIS difference between theſe two airs, on the one part, and, on the other, the great analogy which appeared between them, in many reſpects, led me to preſume, that two cauſes are complicated in reſpiration, of which probably I was hitherto acquainted only with one; and to clear up my ſuſpicions on the ſubject, the following experiment was made.

INTO a jar filled with mercury, and inverted into the ſame, were paſſed twelve inches of air, vitiated by reſpiration; and a thin ſtratum of cauſtic fixed alkali was introduced to the ſurface of the metal. I might have made uſe of lime-water, but the neceſſary quantity of it would have been too conſiderable, and would have impeded the ſucceſs of the experiment.

THE effect of the cauſtic alkali was to occaſion a diminution in the volume of this air, of nearly $\frac{1}{6}$; while, at the ſame time the alkali had, in part, loſt its cauſticity, and acquired the property of effervеſcing with acids; it alſo cryſtalliſed within the glaſs, even under a very regular rhomboidal form; properties which we know could not be communicated to it, but by combining it with that ſpecies of air or gas, known by the name of fixed air: from whence it appears that air, vitiated by reſpiration, contains nearly $\frac{1}{6}$ of an aeriform acid, perfectly ſimilar to that obtained from chalk.

The air, thus deprived of its fixable part by the cauſtic alkali, was ſo far from being re-eſtabliſhed in the ſtate of common air, that, on the contrary, it approached nearer to that of the air which had been employed in the calcination of mercury, or, more properly, was exactly the ſame; for neither candles could burn, nor animals live in it; nor could I perceive, from any experiments I made, the leaſt difference between the two airs.

Now air which has ſerved for the calcination of metals, is, as we have already ſeen, nothing but the mephitic reſiduum of atmoſpheric air, the highly reſpirable part of which has combined with the mercury, during the calcination: and the air which has ſerved the purpoſes of reſpiration, when deprived of the fixed air, is exactly the ſame; and, in fact, having combined, with the latter reſiduum, about $\frac{1}{4}$ of its bulk of dephlogiſticated air, extracted from the calx of mercury, I re-eſtabliſhed it in its former ſtate, and rendered it equally fit for reſpiration, combuſtion, &c. as common air, by the ſame method as that I purſued with air vitiated by the calcination of mercury.

The reſult of theſe experiments is, that to reſtore air that has been vitiated by reſpiration, to the ſtate of common reſpirable air, two effects muſt be produced: 1ſt. to deprive it of the fixed air it contains, by means of quick-lime or cauſtic alkali: 2dly. to reſtore to it a quantity of highly reſpirable or dephlogiſticated air, equal to that which it has loſt. Reſpiration, therefore, acts inverſely to theſe two effects, and I find myſelf in this reſpect led to two conſequences equally probable, and between which my preſent experience does not enable me to pronounce.

After what we have ſeen, it may be concluded, that one of the two following effects is produced by reſpiration: either the highly reſpirable portion contained in common air, is converted into fixed air, in paſſing through the lungs, or elſe an exchange is made in that viſcus; on one part the dephlogiſticated air is abſorbed, and on the other, the lungs throw out a portion of fixed air, nearly equal in bulk to the other.

The firſt of theſe opinions is ſupported by an experiment which I have already communicated to the academy. For I have ſhewn in a memoir, read at our public Eaſter meeting, 1775, that dephlogiſticated air may be wholly converted into fixed air by an addition of powdered charcoal; and, in other memoirs, I have proved that this converſion may be effected by ſeveral other methods: it is poſſible, therefore, that reſpiration may poſſeſs the ſame property, and that dephlogiſticated air, when taken into the lungs, is thrown out again as fixed air. But, on the other hand, ſtrong analogies ſeem to militate in favour of the ſecond opinion, and lead us to believe that a portion of the pure air remains in the lungs, and is combined with the blood. We know that it is one property of this air to communicate a red colour to bodies, and eſpecially to metallic ſubſtances, with which it is combined: mercury, lead, and iron furniſh examples of this fact. Theſe metals form, with highly reſpirable air, beautiful red calces, the firſt under the name of calcined mercury, or red precipitate of mercury; the ſecond under that of minium; and the third, of colcothar. The ſame effects, the ſame phenomena, are obſervable, in the calcination of metals and in the reſpiration of animals; all the circumſtances are the ſame, even to the colour of the reſiduums. May we not then ſuppoſe that the red colour of the blood depends on the combination of dephlogiſticated air, or, to ſpeak more accurately, of its baſis with an animal fluid, in the ſame manner as the colour of red precipitate of mercury and minium is owing to the combination of this baſis with a metallic ſubſtance? Though this conſequence has not been deduced by Mr. Cigna, Dr. Prieſtley, and other moderns who are occupied on this object, I will venture to ſay that there is none of their experiments that do not tend, in appearance, to confirm it; nay, they have proved, and more eſpecially Dr. Prieſtley, that the blood is red only in proportion as it is continually in contact with atmoſpheric, or with dephlogiſticated air; that it becomes black in fixed air, in nitrous, in inflammable, and in all unreſpirable airs, and alſo in the exhauſted receiver of an air-pump: that on the contrary it recovers its red colour, when placed again in contact with air, and eſpecially if it be dephlogiſticated, and that this reſtoration of colour is conſtantly attended by a diminution in the volume of air. Does it not then follow, from all theſe facts, that this pure ſpecies of air has the property of combining with the blood, and that this combination conſtitutes its red colour. But whichever of theſe two opinions we embrace, whether that the reſpirable portion of the air combines with the blood, or that it is changed into fixed air in paſſing through the lungs; or laſtly, as I am inclined to believe, that both theſe effects take place in the act of reſpiration, we may, from facts alone, conſider as proved,*

1ſt. THAT reſpiration acts only on the portion of pure or dephlogiſticated air, contained in the atmoſphere; that the reſiduum or mephitic part is a merely paſſive medium which enters into the lungs, and departs from them nearly in the ſame ſtate, without change or alteration.

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* THAT truly ingenious philoſopher Dr. Crawford has proved that dephlogiſticated air contains a large proportion of abſolute heat, and on this fact has founded a very probable theory of the cauſe of animal heat. He ſuppoſes that this pure part of atmoſpheric air has a greater affinity to phlogiſton than to heat, and therefore that when a quantity of atmoſpheric air is inſpired by the lungs, the pure part receives the phlogiſton which is continually throwing off from the blood, by means of the bronchial veſicles, and in return parts with a quantity of abſolute heat, which is abſorbed by the blood. In every proceſs in which there is an emiſſion of phlogiſton, fixed air is formed, and ſeems to conſiſt of phlogiſton united to the pure part of the atmoſphere. T. H.

2dly. THAT the calcination of metals, in a given quantity of atmoſpheric air, is effected, as I have already often declared, only in proportion as the dephlogiſticated air, which it contains, has been drained, and combined with the metal.

3dly. THAT, in like manner, if an animal be confined in a given quantity of air, it will periſh as ſoon as it has abſorbed, or converted into fixed air, the major part of the reſpirable portion of air, and the remainder is reduced to a mephitic ſtate.

4thly. THAT the ſpecies of mephitic air, which remains after the calcination of metals, is in no wiſe different, according to all the experiments I have made, from that remaining after the reſpiration of animals; provided always, that the latter reſiduum has been freed from its fixed air: that theſe two reſiduums may be ſubſtituted for each other in every experiment,

ment, and that they may each be reſtored to the ſtate of atmoſpheric air, by a quantity of dephlogiſticated air, equal to that of which they had been deprived. A new proof of this laſt fact is, that if the portion of this highly reſpirable air, contained in a given quantity of the atmoſpheric, be increaſed or diminiſhed, in ſuch proportion will be the quantity of metal which we ſhall be capable of calcining in it, and, to a certain point, the time which animals will be capable of living in it.

The limits which I have preſcribed to myſelf in this memoir, will not permit me to enter on many other experiments which tend to the ſupport of the theory I have advanced. Of this number, is a part of thoſe which Meſſrs. Trudaine, de Montigny, and myſelf have lately made, in the Montigny laboratory. There is reaſon to hope that theſe experiments will throw ſtill additional light, not only on the reſpiration of animals, but alſo on combuſtion: operations which have a much ſtronger relationſhip to each other, than we may, at firſt ſight, believe.

ESSAY II.

ON THE COMBUSTION OF CANDLES IN ATMOSPHERIC AIR AND IN DEPHLOGISTICATED AIR.

IT having been ſufficiently proved that the air of the atmoſphere is not a ſimple ſubſtance or element, as the ancients believed, and has been imagined even in our own time; but that it is compoſed in part only of very pure or dephlogiſticated air, and that the remainder, which is perhaps ſtill a compound, is mephitic, and incapable of ſupporting either animal life, combuſtion, or flame; it will be neceſſary, in order that this eſſay may be intelligible, to diſtinguiſh four ſpecies of aëriform fluids.

1ſt. ATMOSPHERIC air, in which we live, reſpire, &c.

2dly. Pure or dephlogiſticated air, which forms about $\frac{1}{4}$ of the compoſition of atmoſpheric air.

3dly. The mephitic part of atmoſpheric air, which forms $\frac{3}{4}$ of its compoſition; with the nature of which we are as yet wholly unacquainted.

4thly. Fixed air, as it is commonly called, which may exiſt either in a ſtate of elaſticity, or quieſcent and united with other bodies.

Those who have been employed in making experiments on the burning of candles, have been convinced that a conſiderable diminution took place, during the combuſtion, in the volume of air. To prove this, a very ſimple but inconcluſive experiment has been made. A lighted candle has been placed on the plate of an air-pump, and being covered with a receiver, it has been obſerved that the candle was ſoon extinguiſhed, and that as ſoon as the veſſels were become cold, the receiver adhered to the plate. But this effect could not be produced except the volume of air, remaining in the receiver after the combuſtion, were ſmaller than it was previous to the introduction of the candle. But attention has not been paid to this circumſtance, that a candle cannot be placed under a receiver, but the contained air muſt be heated at the inſtant when the candle is introduced, and before the receiver can be applied to the plate. The air included in the receiver therefore being hot, it will diminiſh in bulk as it grows cool; and this may account for the adheſion of the receiver to the plate, when the candle is extinguiſhed and the veſſels cooled.

It is proper alſo to remark, that there are few air-pumps which do not admit of the paſſage of ſome portions of air, between the leather and the edges of the receiver; eſpecially when the receiver, ſo far from adhering to the plate, is rather puſhed from it, by the rarefaction of the internal air; conſequently, there is almoſt always an eſcape of air while the candle continues to burn; and as there will not remain a ſufficient quantity under the receiver to balance the preſſure of the atmoſphere, another cauſe is formed of the receiver's adhering to the plate.

Experiments made in jars immerſed in water are equally unſatisfactory. 1ſt. The air is rarefied when the candle is introduced, and continues to dilate during the combuſtion, and, therefore, a conſiderable portion of it eſcapes at the rim of the jar. Hence it is impoſſible to aſcertain the quantity of air on which we operate, nor the real amount of the diminution in its bulk. 2dly. The burning of candles has the property of changing a part of the atmoſpheric air into fixed air; and as the latter is capable of combining with water, ſuppoſing a diminution in the bulk of the air to have been cauſed by the combuſtion, and taken place during the experiment, it is impoſſible to diſtinguiſh it from that which proceeds from the combination of the fixed air with the water.

These reflections induced me to take another method, and convinced me of the neceſſity of operating over mercury. I accordingly began by immerſing a glaſs jar, at the ſame time inclining it to a certain angle, into a baſin of mercury; and then ſetting it upright, I marked the place which anſwered to the ſurface of the quickſilver, and repeated this trial ſo often, that I was aſſured that the mercury correſponded very nearly each time with the mark I had firſt made on the jar.

Being thus convinced of the practicability of always incloſing the ſame quantity of air under the jar, by ſufficient care and attention; I proceeded in the ſame manner, holding the jar inclined and partly plunged in quickſilver in my left hand, and, with my right, introducing under it, very quickly, a ſmall lighted wax candle. The introduction of the candle, the immerſion and elevation of the jar, ought to be performed in an inſtant, and it will be neceſſary to practiſe theſe evolutions, till ſuch a degree of dexterity be acquired by the experimenter, as to enable him to perform all theſe operations in an almoſt indiviſible ſpace of time.

In a few moments after the candle has been incloſed in the jar, it begins to give a weaker light, and, in a ſhort time, is extinguiſhed. The mercury, as might be expected, deſcends, at firſt, rapidly, owing to the heat and rarefaction of the confined air; but when the candle is extinguiſhed, and the veſſels are perfectly cooled, it returns pretty exactly to the mark which had been made before the introduction of the candle. I have uſed the expreſſion *pretty exactly*, as it is impoſſible to anſwer for very trifling differences in this experiment, becauſe that ſlight errors, as to the height of the mercury, may be cauſed by ſome little variation in inclining the jar more or leſs, or by ſome circumſtances attending its elevation.

The reſult of this experiment did not convince me that the burning of a candle occaſioned no ſenſible diminution in the volume of air; it was ſtill neceſſary to determine the ſtate of the air after the combuſtion, and the changes that might have been effected in it. I therefore introduced to the air in the jar, in which the candle had been extinguiſhed, a ſmall portion of a cauſtic lixivium of fixed alkali. The volume of air was preſently diminiſhed, and reduced from 26 to $23\frac{1}{5}$ inches; ſo that the diminution amounted to nearly $\frac{1}{9}$ of the original quantity of air. At the ſame time the cauſtic alkali had acquired the property of effervеſcing with acids; which proved that the diminution had been occaſioned by the fixed air combining with the alkali: for when I added a ſmall portion of vitriolic acid to the alkaline ley in the jar, a briſk effervеſcence enſued, the abſorbed air was again ſeparated, and the mercury redeſcended almoſt exactly to the mark which I had made on the jar.

Though this experiment was perfectly concluſive in ſome reſpects, it was ſtill inſufficient to my views, relative to the diminution of the bulk of air by combuſtion, and there remained ſtill ſomething incomplete on this ſubject. For conſiderable differences in the experiment might be cauſed merely by inclining the jar more or leſs, and it was very poſſible that the diminution of the air might have been counterbalanced by ſome error in the proceſs; I therefore reſolved to take every precaution in my power, to obtain a reſult that ſhould be more certain, and free from every kind of error; to which purpoſe the following experiment appeared to me likely to be more deciſive.

In the middle of a glaſs ſtand, was placed a ſmall wax candle; and on the top of the wick, was fixed a ſmall piece of Kunckel's phoſphorus, weighing about $\frac{1}{10}$ of a grain. The ſtand was then placed in a baſin of mercury and covered with a jar, and, by means of a ſyphon, I raiſed the mercury, by ſuction, to a certain elevation which I marked exactly with a ſlip of paper. Every thing being thus diſpoſed, I made a piece of iron wire red hot, having firſt bent it for the purpoſe, and then paſſed it through the mercury, into the jar, ſo as to touch the top of the candle and ſet fire to the little piece of phoſphorus. Though the wire muſt, neceſſarily, have been much cooled in paſſing through the mercury, it ſtill retained ſufficient heat to inflame the phoſphorus, and by this means the candle was lighted, as I expected.

WHILE the candle continued to burn, the air was rarefied; but when it was extinguiſhed, the mercury remounted inſenſibly, in proportion as the veſſels cooled, and became ſtationary a little above the mark I had made before the candle was lighted. From this it was evident, that a ſmall diminution in the bulk of the air was effected, and being meaſured very preciſely, it was found to amount exactly to $\frac{3}{4}$ of a cubic inch. But one grain of phoſphorus abſorbs, in combuſtion, three cubic inches of air, as has been eſtabliſhed by ſeveral experiments.* The $\frac{1}{16}$ of a grain therefore ſhould abſorb half an inch, which reduces the real diminution of the air, cauſed by the burning of the candle, to $\frac{1}{4}$ of an inch. The jar contained ſeventy-two cubic inches. On the ſuppoſition, therefore, that the diminution of $\frac{1}{4}$ of an inch was not to be attributed to ſome ſlight error with reſpect to the meaſures, the diminution occaſioned by the burning of a candle in common air would not amount to above $\frac{1}{288}$, which may be regarded as a mere nullity, eſpecially if it be conſidered that a very trifling change in the temperature of the room, is capable of producing this difference.

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* SEE Lavoiſier's Eſſays, Phyſical and Chemical, tranſlated by Henry, chapter ix.

As the jar which I had uſed in this experiment was very long and narrow, I imagined that the candle might not have burnt ſo long as it would have done, if the veſſel had been leſs lofty, and the circulation of air more eaſy. The experiment was therefore repeated in a veſſel which was ſhorter and not capable of containing above thirty cubic inches.

The circumſtances, under which the experiment was conducted, were exactly the ſame as in the former one; and when the veſſels were quite cold there was a diminution of the volume of air of $\frac{1}{2}$ an inch, which preciſely correſponds with the quantity which might be ſuppoſed to be abſorbed by $\frac{1}{16}$ of a grain of phoſphorus, if burnt alone under a jar. The burning of the candle, therefore, had occaſioned no ſenſible diminution in the bulk of air.

From theſe ſeveral experiments it may be regarded as certain, 1ſt. that the burning of candles does not ſenſibly diminiſh the volume of air in which they burn. 2dly. That this combuſtion has the property of converting into fixed air about $\frac{1}{10}$ of the original quantity of air. 3dly. That if the air in which a candle has burnt, be brought into contact with lime-water or cauſtic alkali, in that caſe the above diminution of $\frac{1}{10}$ is produced by theſe liquids abſorbing the fixed air which has been formed during the combuſtion.

The air in which candles have thus burnt, when deprived by water, or any other means, of the portion of fixed air, formed in it, is called by Dr. Prieſtley and ſeveral others, the phlogiſticated part of the air. They are of opinion, that there ariſes from candles when burning, and from metals when calcining, &c. a phlogiſtic emanation which combines with and ſaturates the air. My ſentiments of the caſe are different, and I have already given ſome proofs that the reſiduum of atmoſpheric air, after combuſtion, &c. is its mephitic portion, which forms three fourths of its compoſition, deprived in a greater or leſs degree of its pure, reſpirable part. And in fact, if we reſtore to the reſiduum of the original quantity of air this $\frac{1}{10}$ of pure air which it has loſt, we thereby reſtore it to its former ſtate. Now if, as Dr. Prieſtley ſuppoſes, this air were phlogiſticated, or contaminated by ſome principle which rendered it unſalutary, it would not be ſufficient to reſtore to it the portion of which it had been deprived, but, in order to re-eſtabliſh it in the ſtate of common air, it would be neceſſary alſo to ſeparate this contaminating ſubſtance from it. Beſides, as I am going to oppoſe, by a train of experiments, Stalh's doctrine of phlogiſton,* the objections which I ſhall adduce on that occaſion will militate equally againſt the ſuppoſed phlogiſtication of air.

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* See the preface.

From my account, atmoſpheric air conſiſts of about $\frac{1}{4}$ of its bulk of pure reſpirable air. The combuſtion of candles converts only $\frac{1}{10}$ of it into fixed air; ſuppoſing, then, the volume of air to have been 100 before the combuſtion, there ſhould remain, after the operation, ſeventy-five parts of mephitic, and fifteen of pure air: and we accordingly find that animals will live, or phoſphorus burn, in this reſiduum, after a candle has been extinguiſhed by it; and even after this proof, there will ſtill remain five parts of pure air. This laſt portion is ſo firmly united to the mephitic part, that I know of no other means of ſeparating it than by the combuſtion of pyrophorus; which will appear in a ſucceeding eſſay.

There remains nothing to complete what I have to ſay on the ſubject, except an account of the phenomena which attend the burning of candles in very pure air. Theſe experiments will furniſh me with freſh weapons againſt the gratuitous doctrine of the phlogiſtication of air.

Into a glaſs jar, filled with very pure air, obtained from calcined mercury, a candle was introduced. This jar was fixed in a baſin of quickſilver. The candle burnt with a vivid light, an enlarged flame, and all the circumſtances deſcribed by Dr. Prieſtley. The heat, during the combuſtion, was ſo great, that a portion of air paſſed by the rim of the jar, and eſcaped, but the quantity was not very conſiderable. When the candle was extinguiſhed and the veſſels cooled, I introduced ſome cauſtic fixed alkali, over the ſurface of the quickſilver in the jar. Immediately the fixed air was abſorbed, and I found by this trial that two thirds of the pure air had been converted into fixed air by the combuſtion. But, what was ſtill more intereſting, the remaining third, after the abſorption of the fixed air, was ſtill almoſt pure: for having transferred it into a ſmaller jar, a candle was again lighted in it; it afforded an enlarged flame, and about one half of it was converted into fixed air. The remainder, after the abſorption by cauſtic alkali, was nearly of the ſame goodneſs as common air.

WHEN, therefore, a candle is made to burn in a jar containing one hundred parts of pure or dephlogiſticated air, ſixty-ſix parts are changed into fixed air, and of the thirty-four remaining parts, twenty-one and $\frac{1}{4}$ are ſtill pure air, capable of being changed into fixed air. Out of 100 parts, only $12\frac{3}{4}$, or about $\frac{1}{8}$, remain of air capable of extinguiſhing a candle without producing any precipitation in lime-water, and this appears to have been a portion of mephitic air contained in the dephlogiſticated air. Undoubtedly this portion would have been ſtill ſmaller, if the air had been more pure.

Now if, as is ſuppoſed by the celebrated philoſopher, Dr. Prieſtley, combuſtion were poſſeſſed of the property of phlogiſticating air, the quantity of phlogiſticated air, formed during the proceſs, ſhould be greater in proportion to the quantity of combuſtible matter conſumed. For the combuſtion is almoſt four times as much in dephlogiſticated, as in an equal bulk of common, air; and therefore four times as much phlogiſticated air ſhould be formed; whereas on the contrary we obtain nine times leſs. The diſproportion, then, of what we really have, to that which we ought to expect, according to Dr. Prieſtley's opinion, is as 1 to 36.

Laſtly, the reſiduum after the combuſtion of phoſphorus, and eſpecially of pyrophorus, in dephlogiſticated air, is ſtill leſs than that after the burning of candles, and may be conſidered as a mere nothing; whereas, according to Dr. Prieſtley's theory, it ought to be more conſiderable. It cannot therefore be to the addition of phlogiſton that the formation of the mephitic reſiduum of atmoſpheric air after combuſtion is to be attributed, and it therefore muſt have exiſted, as I have advanced, previous to the combuſtion.

The preceding experiments ſeem therefore to have proved the following facts:

1ſt. That the mephitic portion which forms $\frac{3}{4}$ of the compoſition of atmoſpheric air, contributes nothing to the phenomena of combuſtion.

2dly. That the action of combuſtion is confined to the portion of pure or dephlogiſticated air, which forms the other $\frac{1}{4}$ of atmoſpheric air.

3dly. That only $\frac{2}{5}$ of this pure air can be converted into fixed air, by the burning of candles, and that the other $\frac{3}{5}$ remain united to the mephitic portion, without combuſtion having the power to ſeparate them,

4thly. That phoſphorus has a greater power than candles, as it is capable of ſeparating $\frac{4}{5}$ of the pure air contained in atmoſpheric air,

5thly. That pyrophorus acts yet more forcibly, and ſeems to convert almoſt the whole portion of pure, into fixed air.

These conſequences might be carried much further, and it might be ſhewn, that fixed air, which is formed during the combuſtion of candles, is nothing but the inflammable air ſeparated from the candle, united with a greater proportion of the pure part of the air in which the combuſtion is carried on, and a ſmaller, though conſiderable, portion of the matter of fire which enters into the compoſition of both kinds of air. But the proofs which might be adduced of theſe aſſertions are ſuch as my readers are not yet prepared to receive, and I muſt defer the elucidation of this theory, till, on one part, I have demonſtrated the exiſtence of the matter of fire in aëriform fluids, and have ſhewn in a future eſſay, that fixed air may be formed, by combining inflammable, with the baſis of dephlogiſticated, air.

ESSAY III.

ON THE COMBUSTION OF KUNCKEL's PHOSPHORUS, AND ON THE NATURE OF THE ACID RESULTING FROM THE PROCESS.

SECTION I.

On the Combuſtion of Phoſphorus, and the Formation of its Acid.

I HAVE already related, in chap. IX. part II. of my Eſſays Phyſical and Chemical, ſome of the principal phenomena attending the combuſtion of phoſphorus, and the formation of its acid; but the intelligence I have acquired, ſince the publication of that work, enabling me to give a more accurate account of the reſults, and to be more certain in my explanations, I ſhall reſume the ſubject in a ſummary way, and ſhall firſt ſpeak of the formation of the phoſphoric acid, before I proceed to the relation of the reſults of its combination with different mineral and vegetable ſubſtances.

If Kunckel's phoſphorus be lighted, by means of a burning glaſs, under a jar inverted into a baſin of mercury, we obſerve, 1ſt. that only a given quantity of phoſphorus can be burnt in a determined quantity of air, and that theſe proportions are about one grain of the former to from ſixteen to eighteen cubic inches of air.

2dly. That as ſoon as this quantity is burnt, the phoſphorus is extinguiſhed, without any poſſibility of kindling it again, except by bringing it into contact with freſh air, which has not hitherto been employed for combuſtion.

3dly. That freſh phoſphorus, introduced under the ſame receiver, does not burn better than the former.

4thly. That during the combuſtion, a large quantity of white flowers or flakes are formed, which reſemble very fine ſnow, and attach themſelves every where to the inſide of the receiver. Theſe conſtitute the concrete phoſphoric acid.

5thly. That at the beginning of the combuſtion, the air in the receiver is conſiderably dilated, on account of the heat which is produced; but, in a very ſhort time, its bulk becomes ſo much diminiſhed, that, when the veſſels are cooled, it occupies no more than about $\frac{4}{5}$ or $\frac{5}{6}$ of the ſpace it filled previous to the combuſtion. If the flowers are collected and carefully weighed, before they come in contact with freſh air, or have attracted any moiſture from it, they will be found to be $2\frac{1}{2}$ times the weight of the phoſphorus employed for their formation; or in other words, with one grain of phoſphorus there will have been formed $2\frac{1}{2}$ grains of concrete phoſphoric acid.

This very extraordinary increaſe of weight is pretty exactly proportioned to the quantity of air abſorbed; for the abſorption is actually about three cubic inches of air for each grain of phoſphorus that has been burnt; and as three cubic inches of air weigh about one grain and half, this weight, added to one grain of phoſphorus, gives $2\frac{1}{2}$ grains, the weight which the flowers have been obſerved to poſſeſs.

The air thus reduced, as much as it can be, by the combuſtion of phoſphorus, is ſo far from being more denſe than atmoſpheric air, that its ſpecific gravity is even rather diminiſhed than augmented. It is no longer capable of ſerving for the reſpiration of animals, of ſupporting the combuſtion or inflammation of bodies, and, in ſhort, is abſolutely in a mephitic ſtate, and therefore to avoid confounding it with any other ſpecies of air, I ſhall diſtinguiſh it, in this memoir, by the name of atmoſpheric gas: but if to this air, thus decompoſed, and which no longer poſſeſſes the principal characteriſtics of common air, be added a quantity of dephlogiſticated or highly reſpirable air, obtained from the calx of mercury or of lead, equal to the bulk of air which was abſorbed during the combuſtion, it is again rendered capable of ſupporting the reſpiration of animals, the combuſtion of bodies, &c. and in a word re-aſſumes all the properties it poſſeſſed before the operation.

If after the above deſcribed re-eſtabliſhment of the air, the phoſphorus be kindled again, exactly the ſame effects are obſerved as in the former combuſtion; and the air may be repeatedly reduced to a mephitic ſtate, and reſtored to that of common air, by the ſame methods.

It is however proper to obſerve, that if we wiſh to carry this experiment to ſome length, it will be neceſſary to add, each time, a ſomewhat larger proportion of dephlogiſticated air, than has been abſorbed in the preceding combuſtion; becauſe this air is never perfectly pure, but always contains a ſmall portion of atmoſpheric gas, ſo that the quantity of the latter will be ſomething increaſed, though very inconſiderably, during each combuſtion; and this increaſe will be leſs perceptible in proportion to the purity of the highly reſpirable air employed in the proceſs; nay, we may arrive at ſuch preciſion, in making the experiment, as to be able previouſly to determine the quantity of this pure air which will be required, according as its degree of purity has been aſcertained by the teſt of nitrous air.

We know, from experiments, of which I have already given an account, that atmoſpheric air contains about $\frac{1}{4}$ of dephlogiſticated or highly reſpirable air. The abſorption, however, which takes place, during the combuſtion of the phoſphorus, never exceeds, and is generally leſs than, $\frac{1}{5}$, from whence it is evident, that the whole of this pure air, contained in the atmoſpheric air, is not ſeparated by the combuſtion: frequently the remainder which is left mixed with the atmoſpheric gas, is $\frac{1}{12}$, and ſometimes more; ſo that this air, which has been drained and rendered noxious by the above proceſs, when it has been well waſhed, is capable of becoming again reſpirable, of ſupporting flame, &c. The following relation contains what has been done on this ſubject.

THOUGH the mephitic portion of atmoſpheric air does not eaſily combine with water, yet this combination is, in ſome degree, effected, if it be agitated for a conſiderable length of time in a large quantity of water. The highly reſpirable air is ſtill leſs diſpoſed to unite with that fluid. If therefore we agitate in the above manner the air in which phoſphorus has been burnt, and which is compoſed, as has been ſeen, of $\frac{1}{12}$ of highly reſpirable air, and $\frac{11}{12}$ of atmoſpheric gas; the highly reſpirable air, which at firſt formed only $\frac{1}{12}$, will be found to amount to a larger portion, and this will increaſe in proportion as the atmoſpheric gas is abſorbed by the water. The air then which has ſerved for the combuſtion of phoſphorus, being waſhed and well agitated in a large quantity of water, muſt paſs through all the intermediate ſtates from that to which it was reduced by combuſtion, to that of dephlogiſticated air. But the change cannot be effected, without a diminution of its volume, which is, for the moſt part, made at the expence of the noxious part.

The whole of this theory, relative to the combuſtion of phoſphorus, and the formation of its acid, is equally applicable to the combuſtion of ſulphur, and the formation of the vitriolic acid; with this difference only, that the combuſtion of ſulphur, not being ſo eaſily ſupported, and this ſubſtance extinguiſhing more readily than phoſphorus, it is more difficult to deprive a given quantity of air of its highly reſpirable part, by the one than by the other; accordingly, as ſoon as $\frac{1}{10}$ or $\frac{1}{8}$ of the air is conſumed, the ſulphur ceaſes to burn; whereas bodies ſuch as phoſphorus, which are more combuſtible, and more ſuſceptible of being kept in a ſtate of ignition, will burn in it longer. By this difficulty I have been prevented from obtaining ſuch exact reſults from the combuſtion of ſulphur as from that of phoſphorus, and I have therefore omitted, at preſent, to give the detail of them. But thus far I can aſſert, that if ſulphur be burned in a glaſs jar, inverted into mercury, the diminution in the volume of air will be proportionable to the conſumption of ſulphur, and that a very concentrated vitriolic acid will be produced, which will be two or three times the weight of the ſulphur employed to form it. This is a ſubject which I ſhall in future reſume, and ſhall beſtow upon it all poſſible accuracy.

I FLATTER myſelf that I am ſufficiently authoriſed by the event of the preceding experiments, both on phoſphorus and ſulphur, to declare, 1ſt. that atmoſpheric air is compoſed of about $\frac{1}{4}$ of highly reſpirable or dephlogiſticated air, and $\frac{3}{4}$ of mephitic, noxious air. 2dly. That phoſphorus, during its combuſtion, acts only on the highly reſpirable part, leaving the mephitic, which may be conſidered as a mere paſſive medium,

dium, unaltered. 3dly. That the phoſphoric and vitriolic acids are compoſed, of above one half of their weight, of highly reſpirable air. I ſhall hereafter ſhew how theſe two acids may be decompoſed, and in what manner we may recover, by means of combinations, this pure air which forms one of their component parts.

The phoſphoric acid, which is obtained by the above proceſs, diſſolves almoſt immediately on coming into contact with the air; and, by thus running ad deliquium, becomes a highly concentrated, heavy acid, poſſeſſing no more ſmell than concentrated vitriolic acid, reſembling it in its oily appearance, and in every other point.

This is the acid which has been employed in all the experiments to be related in this memoir; but I have obtained it by a method ſomewhat more expeditious and convenient, which conſiſts in burning the phoſphorus under large glaſs jars, on the inſide of which a little diſtilled water has been thrown. As ſoon as the vapours formed by the firſt combuſtion are diſperſed, a freſh quantity of phoſphorus is to be introduced and burnt in the ſame manner, and thus the proceſs is to be carried on for ſeveral days, till a ſufficient quantity of acid be obtained. It may be ſuppoſed that the acid procured by this means will be leſs concentrated than the former, as being diluted with water; but it is in other reſpects exactly of the ſame nature, and may be uſed in every experiment, where a highly concentrated acid is not requiſite.

Having ſhewn in what manner this acid is produced, it remains to purſue it through the different unions which it is capable of contracting. To avoid confuſion, and to facilitate the making of references to the experiments contained in the following part of this memoir, I ſhall divide it into different articles.

ARTICLE I.

Phoſphoric Salt with a Calcareous Baſis.

ON adding phoſphoric acid, drop by drop, to lime-water, the water became turbid, and a white precipitate ſubſided, which at firſt ſight appeared to be ſimilar to that produced by throwing fixed air into it; but on more cloſe examination, the ſediment was found to be of a cryſtalline form, a true neutral ſalt, not efferveſcing with acids, diſſolving entirely in water, but requiring a much larger portion to keep it diſſolved, than is requiſite for ſelenite, or even lime. The water on evaporation afforded more of theſe cryſtals, the figure of which was not eaſy to be deſcribed.

A similar ſalt was obtained by adding the acid to quicklime and to chalk. The mixture with the latter was attended with efferveſcence; and in both caſes the ſalt fell to the bottom of the veſſel as faſt as it was formed, owing to its difficult ſolubility.

This ſalt poſſeſſed ſeveral remarkable ſingularities: firſt, in whatever manner it was made, it always retained an exceſs of acid. So far from being able to give it a ſuperabundancy of calcareous earth, the attempt was vain to render it exactly neutral; and, by waſhing, the ſalt, with its ſuperabundant acid, is diſſolved in the water, whereas the exceſs of calcareous earth remains unchanged on the filter.

It diſſolved more readily in water to which ſome phoſphoric acid had been added, than in ſimple water; but, on evaporation, it did not retain this additional acid, which was drained from it by means of brown paper, or waſhed away by water.

Its affinity to calcareous earths was weaker than to alkaline ſalts, on the addition of which the ſame appearances enſued, as when they are added to ſolutions of calcareous ſalts, formed with the other acids.

On adding, drop by drop, a ſolution of ſilver in nitrous acid to a ſolution of this phoſphoric ſalt, a greyiſh ſaline precipitate was immediately formed, which gradually became red, approaching the colour of leys of wine; but if added to a ſolution of quickſilver in the ſame acid, the precipitate was white and powdery.

Neither this ſalt, nor the phoſphoric acid, communicates any particular colour to the flame of ſpirit of wine: whereas phoſphorus itſelf gives ſpirit of wine a light green colour. But it is remarkable that the phoſphorus does not take fire, as long as it is covered by the ſpirit of wine, and thereby defended from contact with the air, but immediately inflames as ſoon as ſo much of the ſpirit is evaporated, as to leave any part of the phoſphorus uncovered.

ARTICLE II.

Phoſphoric Salt with Baſis of Magneſia Alba.

ON throwing, gradually, ſome magneſia alba into weak phoſphoric acid, it was diſſolved with effervescence; but as the ſalt which was formed, poſſeſſed but a ſmall degree of ſolubility, it precipitated, as faſt as it was formed, to the bottom of the veſſel. On adding diſtilled water, the precipitate was diſſolved; and the ſolution being left to ſtand, all night, in a cold place, a great number of very ſmall regular cryſtals of a flattiſh needle-like form, were found in the morning: they were many lines in length, cut off obliquely at each end, and reſembled very exactly, in figure, the cryſtals which are obtained by a very ſlow and inſenſible evaporation of a ſolution of that kind of gypſum called lapis ſpecularis: but on attempting to ſeparate and dry them in a ſtove, they fell into powder. Nor was I able, by ſubſequent evaporations, to procure ſuch regular cryſtals as theſe which were obtained ſpontaneouſly with a large portion of water. This ſalt was immediately decompoſed by vitriolic acid, which, uniting with the magneſia, formed the bitter purging ſalt, commonly called Epſom ſalt.

ARTICLE III.

Phoſphoric Salt with Baſis of Fixed Alkali.

MINERAL fixed alkali was diſſolved with effervescence in dilute phoſphoric acid. The ſolution was not bitter, but rather agreeable to the taſte, ſomething like that of a ſolution of ſea ſalt. The ſalt obtained by this union had not the leaſt exceſs of acid, as is the caſe with almoſt all the other phoſphoric ſalts. It refuſed to form into cryſtals, whatever degree of evaporation was uſed, and whether the ſolution were made ſuperabundantly acid or alkaline. The reſult was, always, a tenacious gummy reſiduum, which roped like thick turpentine, attracted moiſture from the air, and deliqueſced in it.

THE vegetable fixed alkali diſſolved alſo, in the phoſphoric acid, with efferveſcence. When the combination was formed, it was ſet to evaporate, and, when grown cold, cryſtals were obtained of a columnar ſhape, perfectly ſquare, terminated by a pyramid, having alſo four ſides, commonly equal to each other, and correſponding exactly to the four ſides of the cryſtalline column. Hot water diſſolved nearly double the quantity of this ſalt that was capable of ſolution in cold water. The ſalt had a ſmall degree of acidity. Thrown on burning coals it puffed up, but melted with difficulty; but as ſoon as it flowed, it loſt all its ſaline taſte. I have not yet purſued the inveſtigation of this extraordinary circumſtance, which affords a key to ſome intereſting phenomena.

THIS ſalt gives a very ſlight red tinge to the flame of ſpirit of wine; whereas that with the mineral alkali affords no ſuch appearance. They both precipitate ſilver from nitrous acid in form of a white powder, which is diſtinguiſhable from the precipitate produced by marine ſalts in being very much divided, and not collected in flakes or curds. They alſo ſeparate mercury from that acid in form of a yellowiſh white, and lead in that of a white precipitate, the latter being rather ſaline.

ARTICLE IV.

Phoſphoric Ammoniacal Salt.

THE concrete volatile alkali effervefces with phoſphoric acid, and forms with it a neutral ammoniacal ſalt, more ſoluble in hot than in cold water, the cryſtals of which have ſome ſimilitude to alum; but as they are very complicated, it would not be eaſy to deſcribe them without figures.

ARTICLE V.

Phoſphoric Metallic Salts.

DILUTE phoſphoric acid has no action on mercury in the cold. Globules of mercury have been kept ſeveral months in it without the leaſt appearance of ſolution: perhaps if it had been very concentrated and ſtrongly heated, its action might have been ſtronger; but I have not had an opportunity of making the trial.

Poured on a ſolution of ſilver in nitrous acid, it produced no precipitate.

When cold, it had little effect on iron, but when hot, a briſk effervescence enſued. A taper being plunged into the vacant part of the jar in which the combination had been effected, continued to burn as in common air, but with a lightiſh green flame. The ſolution being evaporated, yielded no regular cryſtals, but a ſaline maſs, of a green colour, and very ſoluble in water.

M. Sage has aſſerted in many of his works, that the phlogiſticated alkali, which is uſed for the preparation of Pruſſian blue, is only an alkali ſaturated with phoſphoric acid, and has given his theory an air of probability which may tend to miſlead. If the aſſertion were true, the phoſphoric ſalts with alkaline baſes ſhould form Pruſſian blue when combined with martial vitriol.—But the contrary is the caſe. The precipitate is whitiſh, diſſolves, though with difficulty, in acids, and leaves only a ſmall inſoluble portion, which has only a blueiſh tinge, ſcarcely perceptible.

Such are the experiments which the proviſion of phoſphoric acid I had made, has enabled me to make. I wiſhed to have gone further, and to have repeated the whole of them; but circumſtances having led me to another kind of experiments, and the difficulty of procuring, for a long time, a ſufficient quantity of pure acid, having deterred me from waiting, I have determined to give them to the ſociety as they are; and hope, that, though incomplete, they may be of ſome uſe to chemiſtry, either by eſtabliſhing new facts, or by deſtroying falſe theories. I can anſwer for the accuracy of my experiments.

ESSAY IV.

ON THE EXISTENCE OF AIR IN THE NITROUS ACID, AND ON THE MEANS OF DECOMPOSING AND RECOMPOSING THAT ACID.

IN the firſt volume of my Phyſical and Chemical Eſſays, it was ſhewn, that when Kunckel's phoſphorus was burnt under a glaſs jar inverted into water, about $\frac{1}{5}$ of the air contained under the jar was abſorbed; and that this diminution of air was proportioned to the increaſe of weight in the phoſphoric acid reſulting from the combuſtion.* I therefore concluded that this acid was, in part, compoſed of air, or, at leaſt, of an elaſtic ſubſtance contained in the air. As exactly the ſame phenomena take place in the combuſtion of ſulphur, and the formation of the vitriolic acid, I had equal reaſon to conclude that air alſo enters into the compoſition of the latter.

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* See alſo the third Eſſay in this collection.

These firſt ſteps led me to reflect on the nature of the acids in general, and on examining the circumſtances of their formation, and deſtruction, I thought that I began to diſcover that they were all compoſed, in great meaſure, of air; that this ſubſtance was common to them all, and that they were varied from each other by the addition of different principles peculiar to each acid.

What was at firſt only probable conjecture, was converted to ſufficient certainty when experiment was applied to theory; and I am at preſent enabled to declare poſitively, not only that air, but the moſt pure part of the air, enters into the compoſition of all the acids without exception; and that on this ſubſtance their acidity depends, in ſo much that we may, at pleaſure, deprive them of that quality or reſtore it to them again, according as we take away, or give to them, the portion of air eſſential to their compoſition.

Before I proceed further on this ſubject, it ſeems neceſſary to inform the public, that one part of the experiments contained in this memoir are not properly mine; perhaps, ſtrictly ſpeaking, there is not one of which Dr. Prieſtley may not claim the original idea. But as ſimilar facts have led us to ſomewhat different conſequences, I truſt, that if I ſhould be accuſed of having borrowed my experiments from the works of that celebrated philoſopher, I ſhall, at leaſt, be allowed the merit of the concluſions.

It is a generally known fact that elaſtic vapours are ſeparated from almoſt all ſolutions of metals in acids, which form ſpecies of air, the properties of which vary according to the nature of the acids, by the aſſiſtance of which we are enabled to form them.

These different kinds of air by no means proceed from the metal, as I ſhall have frequent occaſions to ſhew; they are to be attributed to the decompoſition of the acid itſelf; and I imagined that from hence we might be ſupplied with a ſimple method of analyſing the acids. It ſeemed probable, for inſtance, that by diſſolving mercury in nitrous acid, collecting the different elaſtic principles which eſcape from this combination, and attentively obſerving the phenomena which appear from the firſt inſtant of the ſolution, to the point, when the mercury, after having ſucceſſively paſſed through the ſtate of mercurial ſalt and red precipitate, re-appears finally in its metallic form, I ſhould infallibly acquire information concerning the nature of the principles which enter into the compoſition of nitrous acid.

The experiments which I am going to relate might have ſucceeded equally with any other metal; but I gave the preference to mercury, becauſe this metal poſſeſſes the property of being reduced without addition; and I therefore concluded that I ſhould meet with leſs complication in the courſe of my experiments, and ſhould be led, in a more ſimple manner, to the concluſions at which I wiſhed to arrive.

I therefore took a ſmall matras, with a long narrow neck, which I bent in the flame of a lamp, ſo that the extremity might paſs under a glaſs jar, filled with water, and plunged into a veſſel containing the ſame fluid. Into this matras I introduced two ounces of nitrous acid, which ſmoked moderately, the ſpecific gravity of which was to that of diſtilled water in the proportion of 131607 to 100000; and I added two ounces one drachm of mercury, heating the mixture gently in order to accelerate the ſolution.

As the acid was ſtrongly concentrated, the efferveſcence was briſk and the ſeparation of air very rapid. I received the diſengaged air in different jars, that I might examine the varieties which might poſſibly occur between the parcels ſeparated at the beginning, and thoſe at the end of the efferveſcence. When the efferveſcence was finiſhed, and the whole of the mercury diſſolved, I continued to make the ſolution hot in the ſame veſſel. In a ſhort time the efferveſcence was ſucceeded by an ebullition, during which, the production of air continued almoſt as great as at firſt. The proceſs was carried on, till, the whole of the fluid having been converted into air or watery vapours, I had nothing left in the matras but a white mercurial ſalt, of a paſty form, rather dry than moiſt, and beginning to turn yellow on the ſurface. The quantity of air, which had been already obtained, amounted to about 162 cubic inches. All this air was of the ſame nature, and in no reſpect differing from that to which Dr. Prieſtley has given the name of nitrous air.

The operation being continued, I perceived red vapours to ariſe from the mercurial ſalt, ſimilar to thoſe of the nitrous acid; but this circumſtance was not of long duration, and preſently the air, contained in the upper part of the matras,* recovered its tranſparency. Having ſet apart the air, amounting to ten or twelve inches, which had paſſed during the continuance of the red vapours, it was found to be very different from that which had been hitherto collected, and not to differ from common air, except that candles burnt ſomething better in it. At the ſame time the mercurial ſalt was changed into a beautiful red precipitate, and continuing to urge it with a moderate degree of fire, I obtained, in the ſpace of ſeven hours, 234 cubic inches of air, much purer than common air, in which candles burned with a very enlarged flame, and which, by all its characters, convinced me that it was the ſame I had extracted from the calx of mercury, commonly called mercury precipitated per ſe, and which Dr. Prieſtley has obtained from a great variety of ſubſtances, by mixing them with ſpirit of nitre. In proportion as this air was diſengaged, the mercury was reduced, and I recovered, within a few grains, the two ounces one drachm which had been employed in the ſolution. This trifling loſs proceeded from a ſmall portion of a yellow and red ſublimate which adhered to the dome of the retort.

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* These vapours owe their origin to a portion of nitrous air, and of highly pure air, which are diſengaged at the ſame time from the mercurial ſalt, and combining together, form nitrous acid. This explanation will not be well underſtood till the whole of this Eſſay has been read.

The mercury having been recovered from this experiment, in its former ſtate, without change either in its quality or weight, it is evident that the 426 cubic inches of air, which had been obtained, could not have been produced but by the decompoſition of the nitrous acid. I had therefore reaſon to conclude that two ounces of nitrous acid are compoſed, 1ſt. of 190 inches of nitrous air; 2dly. of 12 inches of common air; 3dly. of 224 inches of air better than common air; 4thly. of phlegm. But, as it has been proved, by Dr. Prieſtley's experiments, that the ſmall portion of common air I had obtained could be nothing but air better than common air, the ſuperior quality of which had been altered by a mixture of nitrous air, in the tranſition from one to the other, I may re-eſtabliſh the quantity of theſe two airs, as previous to their mixture, and ſuppoſe that the 12 inches of common air, which I obtained, proceeded from a mixture of 24 inches of nitrous air, and a like quantity of air ſuperior to that of the atmoſphere.

In thus re-eſtabliſhing the quantities, we ſhall have, as the product of two ounces of nitrous acid,

		Inches.
Of nitrous air	-	196.
Very pure air	-	246.
	Total	442.

And for the product of one pound of the ſame acid,

Nitrous

	Inches.
Nitrous air -	1568.
Very pure air -	1968.
Total	3536.

If it were poſſible to know the abſolute weight, as well as the bulk, of theſe quantities of air, it would be eaſy to determine the weight of phlegm; and we ſhould then poſſeſs a complete analyſis of the nitrous acid. The experiments of Dr. Prieſtley to this purpoſe are far from being ſatisfactory, and I confeſs that I have not been able to arrive at any certain concluſions. I ſhall, however, ſuppoſe here, as I have every reaſon to preſume, that the pure air, obtained from mercury, is ſomething heavier than common air, and that it weighs $\frac{55}{100}$ of a grain to the cubic inch. I ſhall alſo ſuppoſe, that nitrous air is ſomething lighter than atmoſpheric air, and that its weight is $\frac{4}{10}$ of a grain to the cubic inch; on this ſuppoſition we ſhall find that a pound of nitrous acid, ſuch as that employed, will be compoſed as follows, viz.

	Oz.	ʒ.	Grs.
Nitrous air -	1		51¼.
Very pure air -	1	7	2½.
Phlegm, or Water	13		18.
Total	℔j.		

Here, then, we are preſented with a method of decompoſing the nitrous acid, and demonſtrating the exiſtence of air, or rather of more pure and perfect air than that of the atmoſphere; but a complete proof of this fact was obtained, when, after having decompoſed the nitrous acid, I was able to recompoſe it by again combining the ſame materials. This is a power which I have actually attained; but previous to the relation of that experiment, it will be neceſſary to give ſome account of the nature of nitrous air.

Those who have not read the experiments related in Dr. Prieſtley's firſt volume on different kinds of air, and eſpecially thoſe of Mr. William Bewley, which are inſerted at the end of that volume, may perhaps imagine nitrous air to be merely nitrous acid in the form of vapour. It will be ſufficient, in order to overthrow this opinion, to make it appear that it is even doubtful whether nitrous air be in a ſtate of acidity, and this the reſult of the following experiments tends to prove.

1ſt. Nitrous Air is capable of paſſing through very conſiderable quantities of water, and rating it with alkali, and evaporating the water, a true nitre was obtained.

With a view of obtaining the acid in a more concentrated ſtate, I endeavoured to ſubſtitute mercury in the place of water, by forming the ſame mixture in a tube filled with, and inverted into, mercury, taking care to leave a ſmall ſtratum of water over the ſurface of the metal. The penetration of the two airs was nearly as rapid in this as in the preceding experiment; the vapours of nitrous acid were condenſed by the ſmall portion of water contained in the tube, and by proportioning properly the quantity of water, I was capable of forming either a very ſmoking ſpirit of nitre, as ſtrong as can poſſibly be made, or one that was more weak, and ſimilar to that originally employed in the operation. This experiment ſhould be made with all poſſible expedition, becauſe the ſmoking ſpirit of nitre, which is formed and comes into contact with the mercury, acts upon the metal, diſſolves it, and again forms freſh nitrous air. This laſt circumſtance furniſhes an additional proof of the recompoſition of the nitrous acid.

It may perhaps appear ſurpriſing, that ſeven parts and $\frac{1}{3}$ of nitrous air, and only four parts of very pure air, are neceſſary to compoſe the nitrous acid; whereas in the decompoſition of it, a ſomewhat larger quantity of pure than of nitrous air was obtained. I am uninformed of the cauſe of this circumſtance, but the fact is certain, that the above proportions of nitrous and pure air exactly ſaturate each other: and conſequently that by employing the very materials, which have been afforded by the nitrous acid in its decompoſition, it is impoſſible to form again the quantity of acid which exiſted before the ſolution, and that a defect appears of nearly one half of the pure air.

Having ſhewn that the principles of the nitrous acid are capable of diſunion and recombination, it remains for me to demonſtrate that we may produce the ſame effect with materials which are not all derived from the nitrous acid. Inſtead of very pure air, procured from the red precipitate of mercury, atmoſpheric air may be employed. But a greater proportion of it will be neceſſary, and inſtead of four, nearly ſixteen parts will be wanting to ſaturate ſeven parts and $\frac{1}{3}$ of nitrous air. The whole of this laſt is deſtroyed or condenſed in this, as in the preceding experiment; but that is not the caſe with the common air, of which not above $\frac{1}{5}$ or $\frac{1}{4}$ is abſorbed, and the remaining part is no longer in a ſtate fit for the ſupport of flame, or the reſpiration of animals. It ſhould appear to be proved, therefore, that the air which we breathe contains only $\frac{1}{4}$ of real air; and that this true air is mixed, in our atmoſphere, with $\frac{3}{4}$ of noxious mephitic air, which would be fatal to moſt animals, were its quantity a little more conſiderable. The deleterious effects of the vapour of charcoal, and of ſeveral other exhalations, are further proofs how nearly this fluid approaches the boundaries beyond which it would become mortal to animals. I hope ſoon to be able to diſcuſs this idea, and to give ocular demonſtration to the academy of the experiments on which it is founded.

FROM the experiments, contained in this memoir, it appears, that when mercury is diſſolved in nitrous acid, the metal attracts the portion of air contained in the acid which conſtitutes its acidity: on one part the metal, combined with the air, is reduced to a calx; and, on the other, the acid, deprived of its air, is expanded and forms nitrous air. That theſe things really happen, during the operation, is proved by the mixture of the two airs, which originally entered into the compoſition of the nitrous acid, again forming pure nitrous acid ſimilar to what it was previous to the ſeparation of its principles.

Nitrous acid then conſiſts of nitrous air combined with $\frac{6}{11}$ of its bulk of the pureſt part of common air, and a conſiderable portion of water. It will, doubtleſs, be aſked, whether the phlogiſton of the metal may not contribute ſomething in the operation? Without preſuming to decide on a queſtion of ſuch great conſequence, I ſhall only anſwer, that as the mercury is left, after the proceſs, preciſely the ſame as before, it does not appear that it has either loſt or recovered its phlogiſton; except it can be ſuppoſed that the phlogiſton neceſſary to the reduction of the metal had paſſed through the veſſels: but this is to admit of a ſpecies of phlogiſton different from that of Stalh and his diſciples, and to return to the principle of fire; to fire combined in bodies; a ſyſtem of greater antiquity than that of Stalh, and very different from it.

I SHALL conclude this memoir, as I began it, by acknowledging my obligations to Dr. Prieſtley for the greateſt part of what intereſting matter it may contain. But the love of truth, and the promotion of ſcience, to which all our efforts ought to be directed, oblige me at the ſame time to take notice of one error into which he has fallen, and to which it might be dangerous to accord. This juſtly celebrated philoſopher having obſerved that, by combining nitrous acid with any kind of earth, he conſtantly obtained common air, or air of even a purer nature than common air, believed that it might therefore be concluded that atmoſpheric air is compounded of nitrous acid and earth. This theory will be ſufficiently confuted by the experiments contained in this memoir. It is evident that it is not the air that is compoſed of nitrous acid, as the Doctor has imagined; but, on the contrary, the nitrous acid which is compoſed of air; and this remark alone ſupplies a key to a great number of experiments contained in Dr. Prieſtley's ſecond, third, fourth, and fifth volumes on air.

ESSAY V.

ON THE SOLUTION OF MERCURY IN VITRIOLIC ACID.

HAVING ſhewn in former eſſays that nitrous acid is the reſult of a combination of a certain proportion of dephlogiſticated, with nitrous, air; that ſulphur and phoſphorus are incapable of acquiring acidity but in proportion as they are combined with a very conſiderable portion of dephlogiſticated air; and having declared that it is poſſible to diſcover, in the vitriolic acid, by chemical experiments, the dephlogiſticated air which entered into its compoſition in the combuſtion of the ſulphur: I ſhall proceed in the preſent eſſay to prove, by means of analyſis, what I have hitherto only accompliſhed by means of compoſition.

Four ounces of mercury, and ſix ounces of vitriolic acid, were put into a ſmall glaſs retort, and gradually heated by an open fire in a reverberatory furnace. The extremity of the retort, the neck of which was very long, was immerſed in a baſin of mercury, and the air, in proportion as it aroſe, was received into tall narrow jars filled with, and inverted into, mercury. The ſolution was effected with conſiderable effervesſcence, during which, a very conſiderable quantity of volatile vitriolic air was ſeparated, which, as long as it is confined in mercury, and does not come into contact with water, preſerves its elaſticity, and is incapable of either rarefaction or condenſation, except by various degrees of compreſſion or heat.

This air, when expoſed to water, is but ſlowly abſorbed, and the combination is attended with ſenſible heat. The water which has been thus impregnated is clear and limpid, and forms what we call volatile, ſulphureous, or vitriolic, acid. But the quantity of this air, abſorbable by water, varies conſiderably, according to the different temperature, and is greateſt when the water is coldeſt; whereas, on the contrary, when the water is nearly boiling, it is incapable of abſorbing a particle of it. I could not poſſibly determine with accuracy how much of this air would be neceſſary to ſaturate a given quantity of water in different degrees of temperature. But thus far is certain, that the water is capable of abſorbing a greater portion of this, than of fixed, air, but much leſs than of marine acid air.

If it be deſired to collect the whole of this vitriolic air, it will be only neceſſary to adapt, to the retort, a tubulated receiver, ſimilar to that deſcribed by Mr. Woulfe and improved by Mr. Bucquet; by which means we catch in the receiver, the ſulphureous acid, in the higheſt ſtate of concentration, and that, which cannot be condenſed, is found united to the water in the bottles connected with the receiver.

The firſt portions of volatile vitriolic air are very pure, but, as the proceſs advances, it becomes mixed with common air, and even with ſome portions of dephlogiſticated air. Theſe may be ſeparated by placing the whole in contact with cauſtic alkali, for the vitriolic air will be inſtantly abſorbed, and the common or the dephlogiſticated air left behind.

If when the mercury is almoſt reduced to dryneſs, the fire be urged rather more briſkly, a ſmall portion of volatile vitriolic air ſtill continues to paſs; but the quantity of dephlogiſticated air which comes over, at the ſame time increaſes continually; and when the reſiduum is quite dry, it is neceſſary to change the apparatus, becauſe the fire neceſſary to the ſucceſs of the operation would melt the retort if it were not placed in ſand.

An aperture was therefore made in a Paris crucible, which might admit the neck of the retort to paſs through it. The crucible was uſed as a ſand bath, in which the retort was placed, having its upper part thinly coated with moiſt refractory clay, to prevent the glaſs being affected by the cold air, as it ought to be equally heated in every part, and alſo that the retort might be leſs expoſed to be broken.

Into an apparatus, ſuch as is now deſcribed, I put two ounces of mercurial vitriol, dry and deprived of the greater part of its water of cryſtallization, and the product of the preceding operation. A briſk fire was applied. The proceſs continued above an hour and half, and during the whole courſe of it, there were collected, 1ſt. a ſmall portion of volatile vitriolic air, which was abſorbed in the water of the tub into which the neck of the retort was immerſed: 2dly. eighty inches of dephlogiſticated air, which was of ſuch a degree of purity, that four parts of it required for its ſaturation ſeven parts or meaſures of nitrous air, and the whole eleven meaſures were reduced to 1 $\frac{1}{8}$. So that the dephlogiſticated air obtained from mercurial vitriol approaches much nearer to abſolute purity than any we have hitherto poſſeſſed.

In proportion to the ſeparation of the volatile vitriolic, and dephlogiſticated air, the mercury, which had been combined with them in the mercurial vitriol, was revived, recovered its form, and paſſed over in diſtillation as running mercury. This however was not wholly the caſe, for two ſpecies of calx of mercury ſublimed into the neck of the retort. The one was white and had a ſaline appearance; the other was grey. I ſhall take an opportunity of examining theſe calxes, which are ſuch as M. Beaumé has declared to be incapable of being reduced without addition. When the operation was finiſhed, nothing remained in the retort.

On calculating the products of this laſt operation, and comparing them with thoſe I had obtained from an equal quantity of mercurial vitriol, in a common apparatus for diſtillation, I found that two ounces of this metallic compound yield,

	℥.	ʒ.	Grs.
1ſt. Of water or phlegm		1	
2d. Running mercury		6	12
3d. White calx of mercury, ſublimed		3	18
4th. Grey calx of mercury			40
Total	1	2	70
The quantity of mercurial vitriol was	2		
The loſs therefore amounted to	0	5	2

This loſs may doubtleſs be accounted for, by the eighty cubic inches of dephlogiſticated air which were collected, and by the volatile vitriolic air which was abſorbed by the water.

It is apparent that as no other ſubſtance was employed in this operation but vitriolic acid and mercury, and as the latter is recovered in its original metallic form, the dephlogiſticated air could only proceed from the vitriolic acid; and, according to the opinion I had advanced, the dephlogiſticated air, which had been abſorbed during the combuſtion of the ſulphur, is found by analyſis in the vitriolic acid.

Another fact, which it is impoſſible to deny, after the experiments that have been related, is, that volatile vitriolic acid is common vitriolic acid, deprived of a part of its dephlogiſticated air.

ESSAY VI.

EXPERIMENTS ON THE COMBUSTION OF ALUM WITH PHLOGISTIC SUBSTANCES, AND ON THE CHANGES EFFECTED ON AIR IN WHICH THE PYROPHORUS HAS BURNED.

AS it will be needleſs to repeat every thing that has been written on the ſubject of Homberg's pyrophorus, and to diſcuſs the various opinions which have, ſucceſſively, prevailed concerning the cauſe of its ſpontaneous inflammation, I ſhall confine myſelf to recurring to a memoir of Mr. Homberg, which is printed among thoſe of the Academy of Sciences for the year 1718, page 238, and eſpecially to that of M. de Suvigny, publiſhed in the third volume of Memoirs of Mathematics and Phyſics, preſented to the academy by learned foreigners.

IT will be proper on this occaſion to recollect, that it has been proved by the experiments of M. de Suvigny, 1ſt. that not only alum, but alſo all the vitriolic ſalts with a baſis of fixed alkali, ſuch as Glauber's ſalt, vitriolated tartar, &c. mixed with a proper proportion of any light porous matter containing phlogiſton, and urged by a degree of fire ſufficient to bring the mixture to a red heat, leave a reſiduum of a blackiſh hue, which has the property of taking fire ſpontaneouſly in the air.

2dly. THAT in all theſe operations the vitriolic acid is converted into ſulphur, ſo that it may be ſaid that Homberg's pyrophorus, and all thoſe formed by M. de Suvigny on ſimilar principles, are merely phlogiſtic hepars of ſulphur with either a fixed alkali or aluminous earth for their baſis.

3dly. THAT it is a proof of the converſion of vitriolic acid into ſulphur in the formation of the pyrophorus, that in whatever manner this ſubſtance be analyſed, not an atom of vitriolic acid, nor of the vitriolic ſalts which had been employed in the operation, is to be found, but only liver of ſulphur and ſulphur.

4thly. That very good pyrophorus may be formed without making uſe of any vitriolic ſalt, by a combination of ſulphur, alkali, and powdered charcoal; a fact which proves moſt incontestably that pyrophorus is a true hepar ſulphuris.—Having ſtated theſe preliminaries, I ſhall proceed to give an account of my own experiments.

Two parts of alum and one part of ſugar were mixed together, and the mixture calcined in an iron ladle, without making it red hot, till the ſugar was entirely converted into charcoal, and neither ſmoke nor vapour continued to riſe.

I took two ounces of this calcined mixture, and placing it in a glaſs retort, in a ſand heat of a reverberating furnace, the fire was raiſed, and the air which ſeparated was received into jars filled with water. About 120 inches of fixed air firſt paſſed, then about 160 inches of air compoſed of equal parts of fixed, and of inflammable air; and the laſt product obtained conſiſted of 180 inches of air, three fourths of which were inflammable, and one fourth only fixed air; and the laſt portions were only pure inflammable air.

The fixed air may be ſeparated from the inflammable, either by ſuffering them to remain for ſome days together in the jars, ſtanding in water, when the fixed gas will be abſorbed, leaving the inflammable, which does not readily unite with water; or the abſorption may be accelerated by placing them in contact with a lixivium of cauſtic alkali, or over lime-water. This liquor entirely and rapidly abſorbs the fixed air, and the inflammable air which remains will be quite pure. By theſe methods I have diſcovered that of 460 cubic inches of elaſtic fluid which were obtained in this operation, 215 were inflammable, and 245 fixed air. But this calculation of the product muſt not be regarded as quite exact, with reſpect to the fixed air, becauſe this acid being obliged to paſs through a conſiderable quantity of water in its way to the upper part of the jar, a part of it was neceſſarily abſorbed by that liquid during the courſe of the operation, and before its bulk could be determined. This circumſtance may be ſuppoſed to have occaſioned a loſs of at leaſt a fourth a fourth or a ſixth of the quantity of fixed air, ſo that the whole that was ſeparated in the proceſs muſt have amounted to, at leaſt, 300 inches.

During almoſt the whole courſe of the experiment, a conſiderable quantity of ſulphur was ſeparated, part of which was ſublimed and condenſed in the neck of the retort, and part, paſſing in vapours through the water, was depoſited on its ſurface in form of a fine powder. The operation continued about an hour and half.

The reſiduum in the retort was Homberg's pyrophorus, was very good and ſtrong, and flamed as ſoon as it came into contact with the air.

The quantities and qualities of the air diſcharged in the operation being ſufficiently determined, I proceeded to the following experiments.

Two drachms of this pyrophorus were placed on the ſcale of a very ſenſible balance, and I obſerved, that it began to increaſe in weight the moment the combuſtion commenced, and that this augmentation continued to take place for ſeveral minutes: in order to diſcover the cauſe on which it depended, it was thought proper to obſerve all the circumſtances of the combuſtion with the utmoſt attention.

I ACCORDINGLY began by introducing ſucceſſively two drams of pyrophorus into different jars filled with fixed air, and with nitrous air. It did not give any light, nor afford any remarkable phenomenon.

BUT the caſe was different when I placed the pyrophorus under jars filled with common, or with dephlogiſticated air; and as the circumſtances of theſe experiments are very remarkable, I ſhall give a full relation of them.

ABOUT half an ounce of pyrophorus was put into a ſmall glaſs bottle, which was covered with a little glaſs cover, and the junctures being luted ſo that the whole apparatus might be paſſed through water without any of the water getting into the bottle, it was conveyed under a jar filled with common air, and I accurately marked the height at which the water ſtood in the jar. Then introducing my hand under the jar, I removed the cover that was affixed to the bottle, and made a free communication between the pyrophorus and the air of the jar. A conſiderable degree of heat was immediately produced without combuſtion, and, at the ſame time, a diminution of the quantity of air took place, which was, at firſt, pretty rapid, but abated in about five minutes; yet did not entirely ceaſe till after the ſpace of between forty-five minutes and an hour had elapſed.

THIS diminution of the volume of air was greater than any I had hitherto obſerved, and bore the proportion of 100 to 72$\frac{1}{2}$, or to more than $\frac{1}{4}$; whereas, in almoſt all operations of this kind, it ſcarcely amounts to a fifth.

WHEN the experiment was made over lime-water, inſtead of common water, the diminution was the ſame; and, as it advanced, I obſerved a precipitation of the lime to be formed, which evinced that one of the cauſes of this diminution was owing to the converſion of a part of the air in the jar, into fixed air, which was abſorbed by the lime-water.

THIS experiment induced me to repeat the experiment in dephlogiſticated or highly reſpirable air; but I perceived the neceſſity of making uſe of a much larger jar, that the phenomena might be more diſtinguiſhable; in other reſpects I conducted the operation much in the ſame manner as before.

As ſoon as the little cover was detached from the bottle, and the pyrophorus came into contact with the pure air, it took fire, and burned with a ſparkling and decrepitation, and particularly with a very ſplendid light and extreme rapidity. In a ſhort time, the violence of the combuſtion abated, and the light began inſenſibly to diminiſh, till, in ſome minutes, it was entirely extinguiſhed.

THE pyrophorus, in this experiment, muſt not be put into a glaſs bottle, but into a ſmall veſſel made of tin, and without ſolder, on account of the great degree of heat which is produced, and would break the glaſs, or melt the ſolder.

AT the firſt inſtant, the great heat produced a ſmall increaſe in the volume of air contained in the jar, but it was preſently ſucceeded by a rapid diminution which alſo abated in about a quarter of an hour, but did not wholly ceaſe till the air was reduced to a ſeventh of the ſpace it occupied previous to the combuſtion of the pyrophorus; nor was it diminiſhed ſo far as it was capable of being, for, when lime-water was admitted, it was again reduced, nearly one half, ſo that the remainder did not amount to above a twelfth or thirteenth part of the original volume of air.

THE remaining parcel of air was ſtill almoſt pure or dephlogiſticated, and by continuing to burn freſh phoſphorus in it, I at length ſucceeded in rendering $\frac{143}{144}$ of the original quantity of air abſorbable by water.

THIS experiment was frequently repeated, and particularly in preſence of Dr. Franklin and ſeveral members of the academy; the circumſtances having been varied by ſometimes employing common water, and ſometimes lime-water; and I am convinced that in the combuſtion of pyrophorus, very pure or dephlogiſticated air is changed into fixed air, except the portion which is abſorbed by the pyrophorus itſelf, as I ſhall preſently ſhew, and that this fixed air combines with the water.

These effects of the combuſtion of pyrophorus in dephlogiſticated air, throw great light on the phenomena attending the combuſtion of that ſubſtance in common air. The effects are nearly the ſame, but with this difference, that atmoſpheric air not containing above $\frac{1}{4}$ of pure, genuine air, no more than that quantity of fixed air is formed and abſorbed. The remaining $\frac{3}{4}$ are the mephitic part of the air, the nature of which is not as yet at all underſtood, and which, as I have demonſtrated on another occaſion, is utterly incapable of ſupporting either animal or vegetable life.

What has been hitherto ſaid, relates only to that portion of pure air which is converted into fixed air, during the combuſtion of the pyrophorus. It remains that I ſhould give an account of ſome circumſtances which, to me, appear to prove, that a conſiderable portion of this air is abſorbed by the pyrophorus, while burning, and combines with it; and that it is the ſurplus only that is changed into fixed air.

And, 1ſt. the diminution of the volume of dephlogiſticated air, at the beginning of the combuſtion of the pyrophorus, is much greater than could proceed from the mere combination of fixed air with water. It is well known that this combination is not, in general, effected very readily, without agitation, whereby the points of contact between the fixed air and the water are multiplied. Theſe circumſtances do not take place under the jar where the combuſtion of the phoſphorus is carried on, and, on the contrary, the great degree of heat which is excited is an almoſt inſurmountable obſtacle to the union of the air and water.

2dly. It is a known fact that pyrophorus increaſes in weight while burning; that this augmentation is very rapid, and nearly equal to the portion of air which we may reaſonably imagine to be abſorbed in this proceſs. It is true that thoſe who have obſerved that pyrophorus thus increaſes in weight, have attributed the increaſe to the moiſture of the air which they ſuppoſed it to attract, and indeed it muſt be allowed, that this is likely to happen at firſt; but when once the pyrophorus is ſtrongly heated, when it is become red and burns with violence, we cannot then ſuppoſe that it attracts moiſture from the air, for it is evident that this great heat muſt, on the contrary, diſpel and reduce it into vapour, if it exiſted in the pyrophorus.

It ſeems certain, therefore, from both theſe conſiderations, that the pyrophorus abſorbs and fixes a conſiderable portion of pure air during its combuſtion.—It may perhaps be aſked what becomes of this air, and what alteration does it produce in the nature of the pyrophorus? This I intend to explain in the remaining part of this eſſay, and hereby to prove, in a more convincing manner, that there is really an abſorption and combination of air in the combuſtion of this ſubſtance.

If pyrophorus be taſted previous to its combuſtion, we perceive nothing of the ſtypticity of alum, but a very diſagreeable taſte of hepar ſulphuris: when, on the contrary, it has burned in pure air, all the coaly part is conſumed, it is perfectly white, it has a part of the ſtypticity of alum, and on diſſolving and evaporating it, we obtain an alum, ſuperſaturated with earth, ſuch as Mr. Beaumé has deſcribed in his Chemiſtry.

THIS laſt obſervation diſcovers to us every thing which happens in the formation and combuſtion of pyrophorus. We clearly perceive that the vitriolic acid of the alum paſſes into the ſtate of ſulphur while the pyrophorus is forming; whereas it changes again to the ſtate of vitriolic acid when the pyrophorus is burning. Now we have been informed by the experiments related in former eſſays, that ſulphur is vitriolic acid deprived of its dephlogiſticated air, or, what is much the ſame, that the vitriolic acid is a combination of ſulphur with that air, or, what will perhaps approach more nearly to accuracy, with the baſis of this pure air. Vitriolic acid therefore cannot be changed from a ſtate of acidity to that of ſulphur, without a ſeparation of dephlogiſticated air being produced, and, on the other hand, ſulphur cannot paſs from its own form to the ſtate of vitriolic acid, without a fixation of this air being effected; and this has been actually obſerved in the experiments related in this eſſay. For we have ſeen that from a mixture of burnt alum and charcoal, amounting to the weight of two ounces, about 400 cubic inches of air were ſeparated, a part of which was fixed, and the other part inflammable air. But the pyrophorus, on the contrary, when in combuſtion, abſorbed a very large quantity of pure air; which facts fully confirm the theory I have advanced. It will undoubtedly be aſked, if the vitriolic acid of the alum contains pure air, why do we principally obtain fixed air during the calcination of the alum and charcoal, and alſo from whence proceeds the inflammable air which paſſes with it? To the firſt queſtion it is anſwered, that the pure air, or its baſis, is converted into fixed air, by uniting with the coaly ſubſtances. Of this we have a proof in the reduction of the calx of mercury: if it be reduced without addition, it yields only dephlogiſticated air; but if charcoal or any phlogiſtic ſubſtance be added, we obtain fixed air.

As to the inflammable air, the quantity ſeparated in this operation is not conſtantly the ſame, but is proportionable to the quantity of charcoal employed. Nor is this air of the ſame nature as that we obtain from the ſolution of certain metallic ſubſtances in the vitriolic and marine acids; it is leſs inflammable, burns with much more difficulty, and makes very little exploſion when mixed with two thirds of common air.

This inflammable air poſſeſſes one remarkable property, viz. that of being changed into fixed air by combuſtion. The other inflammable airs which are obtained by the ſolution of metals, either in the vitriolic or marine acids, differ in this reſpect, and inſtead of being converted into fixed air, at the time they are inflamed, appear to yield acids ſimilar to thoſe by which they have been extracted. Theſe conſiderations, and ſome others which it would be improper to introduce in this eſſay, lead me to ſuſpect that there may be three ſpecies of inflammable air, viz. vitriolic, marine, and fixed inflammable. That which is ſeparated during the combuſtion of the pyrophorus is of this laſt ſpecies. And as this inflammable air produces exactly the ſame effects, in burning, on the pure part of atmoſpheric air, as charcoal does, I am ſtrongly inclined to believe that it is the ſubſtance of the charcoal itſelf reduced to vapour, and in the form of air. For the ſame reaſon the two other inflammable airs appear to me to be, the one, a ſpecies of vitriolic ſulphur, the other of marine ſulphur in an aëriform ſtate. But as my experiments are not quite complete, I muſt content myſelf with this tranſient view of the ſubject.

ESSAY VII.

On the vitriolisation of MARTIAL PYRITES.

THE pyrites which are the ſubject of this memoir, are the common vitriolic martial pyrites, of the moſt common ſpecies, which are frequently found in chalk, and in almoſt all clays, &c. with the nature of which we are well acquainted. But with reſpect to my preſent views, it will be only neceſſary to conſider them as compounded of iron and ſulphur.

If pyrites be diſtilled in an earthen retort, with a red heat, a conſiderable quantity of ſulphur is ſublimed, and collected in the neck of the retort.

If, on the contrary, theſe pyrites remain in a warm moiſt air, they crack on their ſurface, ſplit, fall in pieces, and become covered with vitriolic efflorefcences. And, when they have been expoſed for a ſufficient time to the air, if they be lixiviated, they yield a great quantity of martial vitriol; but if diſtilled in this ſtate, not a particle of ſulphur will be obtained.

THE intervention of air is indiſpenſibly neceſſary to the vitrioliſation of pyrites, which may be preſerved in their original ſtate as long as they can be protected from the action of that fluid. A ſlight covering of oil is ſufficient for this purpoſe; and we find by experience, that they may be kept unchanged under water.

SINCE pyrites, therefore, are compoſed of ſulphur and iron before their efloreſcence, and, after it, of vitriolic acid and iron, it is evident that the ſulphur is converted into that acid, by the effect of the vitrioliſation.

Now as I have declared, and, I truſt, proved, in the eſſays on the combuſtion of ſulphur, and phoſphorus, vitriolic acid is compounded merely of ſulphur ſaturated with very pure or dephlogiſticated air; or in other words, that ſulphur is vitriolic acid deprived of part of its dephlogiſticated air, and vitriolic acid is ſulphur with an over-proportion of the ſame air: the ſulphur of pyrites, therefore, cannot be changed into vitriolic acid without abſorbing pure air.

THE neceſſity of expoſure to the air for vitrioliſing pyrites, was a ſtrong preſumption in favour of the theory; but as it was poſſible to confirm it by experiments, and as, in chemiſtry, we ſhould never be content with reaſoning, when we may prove by facts, I proceeded in the following manner.

I KEPT ſome martial pyrites in a moderately warm place, till they began to effloreſce, when they were immediately removed and placed under a glaſs receiver, plunged in water. The progreſs of the vitrioliſation went on at firſt as rapidly as in the open air; it afterwards abated, and at the end of eighteen or twenty days, was entirely ſuſpended. During all this time the water continued to riſe in the jar, in proportion to the rapidity of the vitrioliſation, and on the eighteenth day began to remain ſtationary.

THE air, in which the pyrites had been confined, extinguiſhed candles, but it neither precipitated lime-water, nor united with alkalies. It was reduced to that ſtate which I have denominated the mephitic portion of the atmoſphere, which had loſt about $\frac{1}{5}$ of dephlogiſticated air; ſo that the pyrites in acquiring a vitriolic property, had abſorbed a portion of pure air, from the atmoſpheric air under the receiver, and the tranſition of the ſulphureous part of the pyrites to vitriolic acid is ſubject to the ſame law, and cannot take place, but by the union of dephlogiſticated air with the ſulphur.

The progreſs of the vitrioliſation of pyrites is much more rapid, if the operation be carried on in dephlogiſticated air. But as I have not purſued this experiment with ſufficient attention, I ſhall poſtpone giving a particular account of it.

The vitrioliſation of pyrites therefore depends on the addition of dephlogiſticated air or its baſis to the ſulphur of the pyrites, whereby the vitriolic acid is formed, which meeting with iron in a ſtate of very minute diviſion, muſt neceſſarily attack and diſſolve it as faſt as it is formed; and from this union proceeds martial vitriol.

ESSAY VIII.

GENERAL CONSIDERATIONS ON THE NATURE OF THE ACIDS, AND ON THE PRINCIPLES OF WHICH THEY ARE COMPOSED.

WHEN the ancient chemiſts had arrived at the power of reducing a body into oil, ſalt, earth and water, they believed that they had attained the utmoſt bounds of chemical analyſis, and they accordingly beſtowed, on ſalt and oil, the appellation of chemical principles.

BUT as the art was continually improving in its progreſs, ſucceeding chemiſts perceived that the ſubſtances which they had been taught to conſider as principles, were ſtill capable of decompoſition; and they ſoon diſcovered that all the neutral ſalts, for example, were formed by the union of two ſubſtances, viz. of ſome acid, with a ſaline baſis of either an earthy or metallic nature.

Hence the whole theory of neutral ſalts, which engaged the attention of chemiſts for above an age, is at preſent brought to ſuch a degree of perfection, that it may be regarded as the moſt certain and complete part of chemiſtry.

In this ſtate, in which the ſcience of chemiſtry has been delivered to us by our predeceſſors, it remains for us to perform, on the *conſtituent parts* of neutral ſalts, what preceding chemiſts have effected on thoſe ſalts themſelves;—to examine the acids and the baſes of which they are compoſed, and to advance this kind of chemical analyſis ſomewhat farther beyond its preſent limits.

In the foregoing eſſays I have endeavoured to prove as clearly as is poſſible by phyſics and chemiſtry, that the very pure air which Dr. Prieſtley has denominated *dephlogiſticated air*, enters, as a conſtituent part, into the compoſition of ſeveral acids, and eſpecially into that of the phoſphoric, vitriolic, and nitrous acids.

Many additional experiments enable me to generaliſe this doctrine, and to declare that this pure and highly reſpirable air, is the conſtitutive principle of acidity; that this principle is common to all the acids; and that the difference by which they are diſtinguiſhed from each other is produced by the union of one or more principles beſides this air, ſo as to conſtitute the particular form under which each acid appears.

These facts being, in my opinion, firmly eſtabliſhed, I ſhall in future diſtinguiſh dephlogiſticated or highly reſpirable air, in a ſtate of combination or fixity, by the name of the *acidifying* principle, or, if any perſon prefer to expreſs the ſame ſignification by a Greek word, the *oxyginous* principle. This name will prevent circumlocution, give more exactitude to my mode of expreſſion, and enable me to avoid thoſe errors, which might be continually occaſioned by uſing the word *air*. For this name, ſince the modern diſcoveries, is become a generic term, and is moreover applicable to ſubſtances in an elaſtic ſtate; whereas at preſent we are to conſider them as combined, and either in a liquid or concrete form.

WITHOUT repeating the particular details which I have before related, I ſhall only in a few words, and in the new terms I have adopted, call back the recollection of the academy to the following poſitions:

1ſt. THAT the acidifying or oxyginous principle, combined with the matter of fire, of heat, and of light, forms pure or dephlogiſticated air. It muſt be allowed, that this firſt propoſition is not ſtrictly proved, and perhaps is not capable of abſolute demonſtration. I have therefore advanced it merely as an idea which I apprehend is very probable; and it muſt not be confounded with the following, which are ſupported by experiment and poſitive proofs.

2dly. THAT the ſame acidifying principle, combined with phlogiſtic ſubſtances or charcoal, forms fixed air.

3dly. THAT with ſulphur it forms vitriolic acid.

4thly. THAT with nitrous air it forms nitrous acid.

5thly. THAT with Kunckel's phoſphorus it forms phoſphoric acid.

6thly. That with metallic ſubſtances in general it forms metallic calces, ſaving the exceptions which I ſhall mention in this or ſome future eſſay.

Thus far then does our preſent knowledge, of the combination of the acidifying principle with the various ſubſtances in nature, extend; and it is not difficult to perceive that there is ſtill a vaſt field left to be traverſed; that there remains a part of chemiſtry which is quite new and hitherto entirely unknown, and which cannot be complete, till we are able to determine the degree of affinity which this principle bears to all the ſubſtances with which it is capable of combining, and to know the different ſpecies of compounds, which reſult from thoſe combinations.

Every chemiſt is ſenſible that the more ſimple the bodies are on which we operate, and the nearer approaches we make in the reduction of ſubſtances to their elementary atoms, the more difficult become the means of their decompoſition, and recompoſition. We know, therefore, that to decompoſe and recompoſe the acids, muſt be attended with much greater difficulties than the analyſis of the neutral ſalts, into the combination of which they enter. I hope, however, to be able to ſhew, in future, that there is no acid, that of ſea ſalt perhaps excepted, which may not be decompounded and recompounded, and which we cannot deprive of the principle of acidity, and reſtore it again whenever we pleaſe.

THIS kind of experiment requires great variety of method; and the proceſſes neceſſary to perform the combination vary according to the different ſubſtances on which we are to operate. In ſome caſes we are obliged to have recourſe to combuſtion, either in atmoſpheric or very pure air, as when ſulphur, phoſphorus or charcoal are the objects. Theſe ſubſtances, during the combuſtion, abſorb the acidifying principle, and by its acceſſion are converted into vitriolic, phoſphoric, and aërial acid or fixed air. With reſpect to other ſubſtances, the mere expoſure to the air, aided by a moderate degree of heat, is ſufficient to produce the combination; and this happens to all vegetable ſubſtances, that are capable of paſſing to the acid fermentation. In every operation of this kind, there is an abſorption of the pure part of the atmoſpheric air, and the acidifying principle forms as many particular acids as there are ſubſtances capable of paſſing to the acid fermentation. After all, we are obliged, in moſt caſes, to have recourſe to the ſcience of affinities, and to employ the acidifying principle which is previouſly engaged in another combination.

The example of this nature, which I deſign to adduce, is one taken from an experiment which has been very well known for ſome years, and related in Mr. Bergman's memoirs, viz. the formation of the acid of ſugar. This acid, according to the experiments which I am going to relate, appears to me to conſiſt only of the ſugar *itſelf* combined with the acidifying or oxyginous principle: and I intend to demonſtrate, in ſeveral ſucceſſive memoirs, that we may, by proceſſes analogous to this, unite this principle with the horn of animals, with ſilk, with animal lymph, with wax, eſſential oils, expreſſed oils, manna, ſtarch, arſenic, iron, and, probably, with many other ſubſtances of the three kingdoms, which are thereby converted into true acids.

Before we proceed farther, it may be neceſſary to recollect that the nitrous acid, as appears from the experiments I have formerly related, is the reſult of the combination of nitrous air with the acidifying principle; that the proportion of theſe two principles varies in different parcels of nitrous acid; that, for example, the ſmoking acid is ſuperſaturated with nitrous air; ſo that we may conſider the ſmoking ſpirit of nitre as a nitrous acid impregnated and overcharged with nitrous air; whereas, on the other hand, that which yields white vapours is ſuperſaturated with dephlogiſticated air. To theſe opinions I ſhall add that from a number of experiments I have made ſince the publication of my former memoir on this ſubject, I am convinced that the nitrous acid, which I have employed on theſe occaſions, and which was conſtantly the ſame, contains about 240 cubic inches of aëriform fluid in every ounce, viz. 120 inches of nitrous, and about an equal quantity of dephlogiſticated air, which is nearly equal in weight to 48 grains of nitrous air, and 60 grains of the acidifying principle to each ounce.—The whole of the remainder conſiſts of phlegm or water.

FROM theſe premiſes it may be always concluded, that whenever I have introduced, into a combination, one ounce of nitrous acid, and have obtained, in the courſe of the operation, 120 inches of nitrous air, there will remain in the combination 120 inches of very pure air, or 60 grains of the acidifying principle. Let us then apply theſe facts to the combination of nitrous acid with ſugar.

INTO a ſmall glaſs retort were put four drachms of ſugar, to which were added two ounces of water, and two ounces of the nitrous acid, I have above deſcribed. The retort was placed over an open fire in a ſmall reverberatory furnace, and to its neck, which was very long, a bottle, with two necks, was adapted, into which were introduced eight ounces, ſeven drachms, and twenty-four grains of diſtilled water. To the ſecond neck of the bottle a glaſs tube was luted, and communicated with a common chemico-pneumatic apparatus ſtanding in water.

It is plain that by theſe diſpoſitions I had a double advantage; on the one hand that of retaining, in the intermediate bottle, whatever might paſs in diſtillation, and be capable of being condenſed: and, on the other, that of receiving in the jars, the different kinds of air which might be ſeparated; ſo that there could be no defect in aſcertaining the whole amount of the products of the operation.

All the junctures were carefully luted with fat lute, which was covered with ſlips of cloth ſpread over with a mixture of white of egg and ſlaked lime; and as ſoon as this exterior covering had become ſo dry as to be ſufficient to ſecure the fat lute, in caſe of its growing moiſt, ſome lighted coals were put under the retort.

At firſt the ſugar diſſolved quietly; but as ſoon as the liquor had acquired forty or forty-five degrees of heat, by Reaumur's thermometer, a very briſk ebullition, or rather effervescence enſued, which proceeded from a very rapid ſeparation of nitrous air, the pureſt and ſtrongeſt I had ever obtained. It is neceſſary to proceed very gradually in this operation, otherwiſe the acid of ſugar itſelf would be decompoſed, and, inſtead of obtaining pure nitrous air, we ſhould have a conſiderable mixture of inflammable and fixed air. It will be neceſſary therefore to withdraw all the fire, the moment the ebullition commences, and to replace it only in ſuch portions as may be neceſſary to keep up the boiling. When the operation is advanced one half or two thirds, the nitrous air no longer paſſes ſo pure; it at firſt becomes mixed with a ſmall portion of fixed air, which keeps continually increaſing, and a little inflammable air; and, when nothing paſſes but theſe two ſpecies of air, the operation may be conſidered as finiſhed.

Having divided, into a great number of portions, the aëriform fluids which had been ſeparated during the proceſs, I proceeded to examine their nature, by the different methods with which we are ſupplied by modern chemiſtry, and I found that the two ounces of nitrous acid, and the four drachms of ſugar, which had been employed, had yielded

		Inches.
Of nitrous air,	- -	190.
Fixed air,	- -	90.
Inflammable air,	-	25.
		305.

When I had ſeparated the veſſels, there were found in the retort, two ounces, ſix drachms, and eighteen grains of a tranſparent colourleſs acid, ſimilar to that deſcribed by Bergman, except that it was liquid; and the water in the intermediate bottle was increaſed in weight one ounce, two drachms and twelve grains, was moderately acid, and had a ſlight nitrous odour.

It will readily be underſtood, that in order to be accurate in the account of the products of this experiment, it was neceſſary that the quality and quantity of the acid, which had been diſtilled and condenſed in the intermediate bottle, ſhould be clearly aſcertained, as no exact calculation could be made without deducting this from the whole quantity of acid employed. For this purpoſe I gradually dropped, into the acid liquor of the intermediate bottle, an alkaline lixivium, compoſed of five parts of water, and four of very pure fixed vegetable alkali. The quantity neceſſary to produce an exact ſaturation amounted to ſix drachms twelve grains. I then determined by another experiment, how much of the original acid would be required to ſaturate an equal portion of the alkaline liquor, and found the quantity to be three drachms fifty-ſix grains and $\frac{1}{3}$. And therefore, of the two ounces of nitrous acid employed, three drachms, fifty-ſix grains had paſſed unchanged into the receiver, and conſequently not above one ounce, four drachms, fifteen grains $\frac{1}{3}$ had really contributed to the other products of the experiment.

Now one ounce of nitrous acid is compoſed, as has juſt been obſerved, of 240 cubic inches of aëriform fluids, viz. of 120 cubic inches of nitrous air, and 120 inches of very pure or dephlogiſticated air, ſo that in the above experiment I had actually combined with the ſugar 183 cubic inches of nitrous air, and the ſame quantity of very pure air, which amount in weight to ſomewhat more than one drachm of nitrous air, and nearly one drachm and half of the acidifying principle. It has been ſeen that during the combination there were ſeparated 190 inches of nitrous air, which amounts to the whole quantity employed: there remained therefore combined 183 inches of pure air; and in the compoſition of two ounces, ſix drachms, eighteen grains of ſaccharine acid remaining in the retort, were contained four drachms of ſugar and 183 inches, or one drachm thirty grains of very pure air, or the acidifying principle; excluſive of the portion of fixed air, which was ſeparated towards the concluſion of the proceſs, and proceeded, as will be preſently ſhewn, from the decompoſition of the ſaccharine acid itſelf.

MR. BERGMAN, and all ſubſequent writers on this ſubject, have therefore been miſtaken in conſidering the ſaccharine acid as the reſult of the decompoſition of the ſugar; for it appears certain, on the contrary, that this acid is formed by the combination of the ſugar with nearly $\frac{1}{3}$ of its weight of the acidifying principle.*

NOT wiſhing to depend on this firſt experiment, I was deſirous of knowing the difference in the reſults, when different portions of ſugar and nitrous acid might be uſed; and I therefore repeated the experiment with the ſame circumſtances, and the ſame quantities of nitrous acid and water, but with only three, inſtead of four, drachms of ſugar, and I obtained

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* IT is, notwithſtanding, probable that the phlogiſton of the ſugar, at leaſt, in part, unites with a portion of the nitrous acid, to form the nitrous air. T. H.

	Inches
Of nitrous air, - -	176.
Fixed air, - - -	127.
Inflammable air, - -	17.
	320.

I impute the greater portion of fixed air obtained in this proceſs to the fire having been raiſed rather more towards the concluſion, and having been continued for rather a longer time, than in the former experiment.

The operation being finiſhed, and the veſſels unluted, the retort was found to contain one ounce, ſeven drachms, forty-eight grains of liquid ſaccharine acid, and proceeding in the ſame manner as before, there appeared to have paſſed by diſtillation, into the intermediate bottle, four drachms, nine grains and half of the nitrous acid originally employed; ſo that the quantity of nitrous acid, which had actually entered into the experiment, amounted to no more than one ounce, three drachms, ſixty-two grains and half; which, according to the above determined proportions, ſhould contain 178 inches of nitrous air, and as much pure air; from whence it appears, 1ſt. that the whole of the nitrous air had been thrown off, the pure air or acidifying principle only remaining. 2dly. That the three drachms of ſugar employed had attracted, during the effervеſcence, eighty-nine grains of the acidifying principle, and that theſe two ſubſtances, when united, and combined with phlegm, formed together the quantity of one ounce, ſeven drachms, forty-eight grains of ſaccharine acid.

Independent of the information, which experiments of this kind afford, upon the nature of acids, they furniſh us with a new method of analyſing animal and vegetable ſubſtances; and though I have nothing quite complete to preſent on this ſubject, I ſhall proceed to give an account of the firſt eſſay which I have made upon ſugar.

In the ſame apparatus that was uſed in the preceding experiments, I mixed two ounces of nitrous acid, with an equal quantity of water and ſix drachms of ſugar, and proceeded in the ſame manner, receiving, in an intermediate bottle, the nitrous acid, which came over in diſtillation. When no more nitrous air was ſeparated, and nothing remained in the retort, but the ſaccharine acid, the veſſels were unluted, the intermediate bottle was removed, and I continued to keep up a gentle fire under the ſaccharine acid, receiving the air, which was ſeparated, directly into jars, till at laſt, having raiſed the fire a little higher, towards the end, there was left only a ſmall coaly reſiduum in the retort. I divided, into ten parts, the elaſtic fluid which had been diſengaged during the operation, and very attentively and accurately ſeparated the different kinds which I had obtained. The following table preſents the whole reſults of the experiment.

An Account, in Cubic Inches, of the Kinds and Quantities of the different Aëriform Fluids obtained in the Combination of Two Ounces of Nitrous Acid, and Six Drachms of Sugar, the Operation being carried on to Drynefs.

Portions.	Nitrous Air.	Fixed Air.	Inflammable Air.	Total.
Firſt	$56\frac{1}{3}$	--	--	$56\frac{1}{3}$
Second	$49\frac{6}{10}$	6	--	$55\frac{6}{10}$
Third	$23\frac{1}{2}$	$22\frac{1}{2}$	$4\frac{2}{3}$	$50\frac{2}{3}$
Fourth	$21\frac{4}{10}$	$30\frac{2}{3}$	$4\frac{1}{3}$	$56\frac{4}{10}$
Fifth	$14\frac{6}{10}$	$45\frac{1}{2}$	$2\frac{9}{10}$	63
Sixth	15	$39\frac{1}{4}$	$7\frac{1}{2}$	$61\frac{3}{4}$
Seventh	--	$28\frac{1}{8}$	$8\frac{1}{4}$	$36\frac{3}{8}$
Eighth	--	48	$11\frac{4}{10}$	$59\frac{4}{10}$
Ninth	--	$32\frac{4}{10}$	8	$40\frac{4}{10}$
Tenth	--	$25\frac{4}{10}$	17	$42\frac{4}{10}$
Totals	$180\frac{13}{30}$	$277\frac{101}{120}$	$74\frac{1}{20}$	$522\frac{13}{40}$

I, AT this time, put no water in the intermediate bottle; and when the operation was finiſhed, there were found in it two ounces and half of weak nitrous acid, which, from the quantity of alkaline liquor ſaturated by it, anſwered to four drachms, ſeventeen grains of the original acid uſed in the experiment. From hence it appears, that only one ounce, three drachms, fifty-five grains of nitrous acid had been decompounded: and this quantity is compoſed, according to the foregoing proportions, of about 177 inches of nitrous air, and an equal portion of pure or highly reſpirable air.

It is apparent from a view of the above table, that the nitrous air which had entered into the combination as one of the conſtituent parts of the nitrous acid, was thrown off in the form of gas, or in an aëriform ſtate; but it may be aſked what is become of the pure air or acidifying principle, which conſtituted the other principle of the nitrous acid? We have ſeen, from the preceding experiments, that it, at firſt, entered into combination with the ſugar, in order to form with it a particular acid; and from the preſent experiment, we learn, that if, after the ſaccharine acid has been formed, the fire be moderately raiſed, the acid is almoſt wholly reſolved into fixed and inflammable air. For what is fixed air? I have demonſtrated, in another place, that it is the reſult of a combination of phlogiſtic matter with the acidifying principle; from whence it ſhould ſeem that ſugar is compoſed of a ſmall portion of inflammable air and much coaly matter. The latter uniting with the acidifying principle of the nitrous acid, forms with it the large quantity of fixed air which is obtained towards the end of the operation. I intend, by further experiments, to illuſtrate any obſcurity which may attend this mode of analyſis.

Every thing I have related as to ſugar, may, as I have already remarked, be applied to a great number of animal and vegetable ſubſtances; from almoſt all of which, by combining them with nitrous acid, or, more properly, with the acidifying principle contained in that acid, we obtain particular acids, many of which have, indeed, ſome properties in common, but afford diſtinct characters in the reſult of their combinations. Here, therefore, we have a new road opened in chemiſtry; and that part of this ſcience which treats of ſalts, to which ſome German chemiſts have given the name of *halotechnic*, inſtead of employing five or ſix acids, is now poſſeſſed of more than double that number, reckoning only thoſe of which I have already acquired a knowledge; and it is not to be doubted, but the number will be, in future, conſiderably increaſed.

From all theſe reflections, and from the experiments contained both in this eſſay and ſome of the preceding ones, it reſults,

1ſt. That when the acidifying principle is combined with any body without decompoſing it, (excepting, however, moſt metallic ſubſtances,) it converts ſuch ſubſtance into a particular acid, which, excluſive of the general properties which are common to all the acids, has others which are peculiar to itſelf.

2dly. That, with reſpect to metallic bodies, it forms, with moſt of them, compounds known by the name of metallic calces. It muſt however be added here, that, even in this claſs of ſubſtances, there are ſome, as arſenic, iron, and perhaps ſeveral others, which, when combined with the acidifying principle, to a certain degree of ſuperſaturation, not only aſſume a ſaline character, but even acquire properties common to the acids, and, like them, become true ſolvents.

3dly. THAT the acidifying principle, like all the others, has its different degrees of affinity; and poſſeſſes, for inſtance, a much ſtronger affinity to ſugar and moſt animal and vegetable ſubſtances, than to nitrous air; and that it is in conſequence of this preference that it quits the latter to form, with different ſubſtances, various kinds of acids.

4thly. THAT the number of acids, which may be formed, is as yet wholly indeterminate, as we are unacquainted with the whole of the ſubſtances which are ſuſceptible of combining with the acidifying principle; and we are ſtill leſs informed of the means which may be uſed to effect this combination.

5thly. THAT the nature of the nitrous acid appearing to be better known, at preſent, than it was formerly; and two diſtinct principles, viz. nitrous air, and pure air, or the acidifying principle, being proved, almoſt to demonſtration, to exiſt in that acid; it affords to chemiſts a valuable means of analyſis, and is capable of throwing conſiderable light on that of vegetable ſubſtances.

6thly. THAT it ſeems not altogether impoſſible, eſpecially from the laſt experiment related in this eſſay, that the phlogiſtic matter is wholly formed in vegetables, and is not, as chemiſts have hitherto imagined, the effect of fire.

ALL theſe concluſions will be illuſtrated and more firmly eſtabliſhed by examples, which I deſign to adduce in a ſubſequent train of memoirs on this ſubject.

ESSAY IX.

ON THE COMBINATION OF THE MATTER OF FIRE WITH EVAPORABLE FLUIDS; AND ON THE FORMATION OF ELASTIC AERIFORM FLUIDS.

I SHALL ſuppoſe in this eſſay, that every part of the planet we inhabit is ſurrounded with a very ſubtile fluid, which penetrates, without any apparent exception, all the bodies which compoſe this globe; that this fluid, which I ſhall call the *igneous fluid*, the *matter of fire*, of heat and of light, has a tendency to place itſelf in equilibrium in all bodies, but is not capable of penetrating all, with equal facility; and that it exiſts, ſometimes in a ſtate of liberty, and ſometimes in a fixed form and combined with other ſubſtances.

THIS opinion, of the exiſtence of an igneous fluid, is ſo far from being new, that it was embraced by many philoſophers among the ancients; and it will therefore be unneceſſary that I ſhould relate the facts on which it is founded: eſpecially as the future memoirs which I have to preſent on the ſubject, will ſerve as proofs of its reality. Indeed if I ſhew that this theory is univerſally agreeable to all the phenomena, and explains all the circumſtances attending experiments in phyſics and chemiſtry, it will almoſt amount to demonſtration.

WHENEVER we form certain combinations in water; when, for example, we unite a fluid acid, that is, an acid diſſolved in water, with a fixed alkali, alſo, in a ſtate of ſolution, a neutral ſalt is formed, and, if the quantity of water be ſufficient, the ſalt is kept diſſolved.

IN every experiment of this kind, the water acts in two diſtinct ways. One part is abſorbed by the ſaline combination, and is called, by chemiſts, the water of compoſition; another part takes the name of the water of ſolution, and keeps the particles of the ſalt ſo equally ſeparated from each other, that every portion of the liquor is equally impregnated with them.

The ſame phenomena prevail relative to the igneous fluid. As all the bodies in nature are immerſed in this fluid, and have imbibed ſome of it, there is ſcarcely any combination whatever but contains a greater or ſmaller quantity of the matter of fire. We ſhould therefore diſtinguiſh, in bodies, between the fire of ſolution, and the fire of combination; between fire in a ſtate of freedom, and fire when combined; in the ſame manner as was above obſerved, with reſpect to water, in the ſolution of ſalts.

After this explanation, it will not be difficult to give a preciſe idea of what we underſtand by heat. Its intenſity is meaſured by the quantity of free and uncombined igneous fluid contained in bodies. Though we have no exact ſcale to *determine* the proportion of igneous matter, we are at leaſt poſſeſſed of methods of *eſtimating* it. Theſe means are ſupplied by the dilatation of bodies, which is effected merely by introducing into them a greater quantity of the matter of fire. Thus, when a thermometer is heated, and made to riſe, we only mix a greater quantity of that matter with the liquor in the tube; for it is not at all ſurpriſing, that in mixing two fluids together, the mixt ſhould occupy a greater ſpace than was held by one of the ingredients only.

With regard to the impreſſion which the igneous fluid makes on our organs—an impreſſion pleaſant and reviving when moderate, but painful and deſtructive when it exceeds certain limits—it proceeds entirely from the tendency in that fluid to combination. We ſhall preſently ſee that beſides the power of penetrating into bodies, and ſeparating their particles, it poſſeſſes the property of converting fluids into vapour, when combined with them in certain proportions; and this converſion into vapour neceſſarily implying a deſtruction of organiſation, muſt conſequently occaſion a painful ſenſation.

Having thus ſufficiently defined what I mean by the matter of fire, or rather by free fire, and by fire in a ſtate of combination, it remains for me to add ſome general reflections upon what muſt neceſſarily happen in the different combinations.

We know that every mixt, every compound, has its own proper portion of igneous fluid; a kind of exact point of ſaturation: for the law of ſaturation ſeems to be general in every phyſical and chemical combination. From whence it follows, that whenever mixts and compounds are brought together, ſo that decompoſitions and new compoſitions are the reſult, one or other of the three following caſes takes place: either the quantity of fire, which enters into the combination, will be the ſame as before, or there will be a decreaſe, or laſtly, an increaſe of it. It is evident, that in the firſt caſe neither a ſeparation or abſorption of the matter of fire is effected; or, in other words, that there is neither any portion of free fire which paſſes into a ſtate of combination, nor any fire that has eſcaped from a combined, to a free, ſtate.

But the ſecond caſe will be different; for a ſmaller quantity of the matter of fire enters into the new combination, than exiſted in the former one; ſo that a part of the igneous fluid, which was combined, previous to the decompoſition, will become free fire after the recompoſition, will recover its properties, and produce the effects which we call heat, and be diſſipated by dividing itſelf inſenſibly among all the ſurrounding bodies, till an equilibrium be formed.

In the third caſe, in which a greater quantity of the matter of fire enters the new, than exiſted in the former, combination; the igneous fluid of the ſurrounding bodies will be abſorbed, and be changed from the ſtate of free, to that of combined, fire. The conſequence of which will be a diminution of free fire in thoſe bodies, which will be perceptible by the degree of cold which takes place; which cold will continue till all the ſurrounding bodies have ſeverally ſupplied the deficient portion of free fire, ſo as to reſtore an equilibrium.

Thus we are furniſhed with a very ſenſible and plain criterion, by which we may determine whether an abſorption or ſeparation of the matter of fire has taken place in any combination. In the firſt caſe, cold will be produced in the ſurrounding bodies; in the ſecond, there will be an augmentation of heat.

We know how impoſſible it is, from the property of free fire to penetrate all ſubſtances, and from its tendency to preſerve an equilibrium, to attain to any great accuracy in this kind of experiments. We can eaſily meaſure, in a jar, the air which is ſeparated during the forming of any combination; but as we have no veſſels which can contain, without loſs, the matter of free fire; as jars, and, in general, all veſſels, are ſo porous as to admit of a free acceſs of it; all we can do is to judge, whether, in a ſhort given time, there be any, or no efflux of the matter of fire; for, if it were poſſible to eſtimate its quantity, it could only be effected by means of a very complicated approximation. I do not, however, deſpair of making ſome uſe, even of theſe methods.

These principles, once eſtabliſhed, it will not be difficult to apply them to the formation of vapours, and of aëriform, or elaſtic fluids, in general.

We have juſt ſeen that whenever there is an abſorption of the matter of fire in any combination, a certain degree of cold is produced in the neighbouring bodies.

Therefore, reciprocally, whenever cold is produced, we may equally conclude, that a portion of free fire has paſſed into a ſtate of combination, or in other words, that the compound has abſorbed a portion of the matter of fire or igneous fluid.

If, therefore, I prove that whenever there is a formation of vapours, cold is produced, I ſhall alſo have proved that there is an abſorption of the matter of fire in the formation of vapours, or, what amounts to the ſame thing, that vapours are the reſult of a combination of the matter of fire, with the fluid, in a highly rarefied ſtate.

I find my only embarraſſment to proceed from the great choice of proofs of this poſition, and I might tranſcribe every thing that has been written on the ſubject of cold, ariſing from evaporation, by Meſſrs. Richman de Mairan, Cullen, and Beaumé.

It will be unneceſſary to give a particular account of the experiments of theſe philoſophers. They merit to be read and attentively conſidered in the original works. It will be ſufficient to remark that they have ſhewn, 1ſt. that if a thermometer be immerſed into any evaporable fluid, and then ſuddenly withdrawn, the thermometer will deſcend, ſeveral degrees, during the time the ball of it is drying, and that it, aſcends again inſenſibly as ſoon as all the fluid is evaporated, till it has recovered the exact temperature of the air and of the ſurrounding bodies in general. 2dly. That as the degree of cold produced is in proportion to the evaporability of the fluid, ſo the deſcent of the thermometer is leſs rapid, and does not proceed ſo far when the bulb is moiſtened with water, as when ſpirit of wine, volatile alkali, or eſpecially æther, is employed. 3dly. That if the evaporation be accelerated by any other means than of heat, the increaſe of cold will be proportionable; ſo that not only all evaporation is attended with cold, but even the cold produced is governed by a certain law proportioned to the rapidity of the evaporation. 4thly. That by continuing to moiſten the bulb of the thermometer, as faſt as it becomes dry, with evaporable fluids, the cold is increaſed more and more, becauſe the efficient cauſe is hereby continued.

If the temperature of all bodies depends, as we have endeavoured to prove, merely on the quantity of the matter of fire, or free igneous fluid which they contain, it will follow that when the bulb of a thermometer is moiſtened with an evaporable fluid, the deſcent of the mercury which enſues is occaſioned by no other cauſe, but becauſe the fluid at the time it is reduced into vapour, carries off from the mercury a part of the matter of free fire which kept it elevated to the degree at which it before ſtood. Fluids, therefore, when they evaporate, deprive the ſurrounding bodies of a part of their matter of fire; and conſequently vapours, and, in general, all aëriform bodies, are compoſed of ſome fluid, diſſolved and combined with the matter of fire.

All the phenomena relative to cold may be rendered more evident, by placing the evaporable fluids in ſuch circumſtances as may be favourable to, and accelerate the formation of vapours; as, for inſtance, in the vacuum of an air-pump. What I am going to relate on this ſubject, is extracted from a very conſiderable work, undertaken jointly by M. de la Place and myſelf, of which the academy has been already informed, and from a memoir which was read at the public meeting, laſt Eaſter.

One experiment alone, which I ſhall relate, goes to prove three points: 1ſt. that the reſiſtance of the atmoſphere is a reſiſtance to be overcome, a force in oppoſition to the evaporation of fluids; 2dly. that as ſoon as this compreſſing power is removed, the evaporable fluids expand and are changed into elaſtic aëriform fluids, or ſpecies of air; and 3dly. that the tranſition of common fluids to a ſtate of elaſticity, is accompanied by an abſorption of the matter of fire, which is taken from all the ſurrounding bodies.

Let a ſmall bottle, or merely a glaſs tube of three or four lines in diameter, be filled with

æther,

æther, and covered with a wet bladder, which is to be faſtened cloſely by paſſing ſeveral rounds of thread about it, and for greater ſecurity a ſecond covering may be faſtened in the ſame manner. The bottle, or tube, is to be ſo filled with æther, as to admit of no portion of air between the liquor and the bladder. Let it then be placed upon a good air-pump, the receiver of which muſt be furniſhed at top with a leathern covercle, penetrated by a ſhank, to which an awl or ſome other ſharp-pointed inſtrument is to be firmly attached, for the purpoſe of pricking the bladder which covers the bottle at the inſtant it appears proper.

Every thing being thus diſpoſed, let the receiver be exhauſted, till the mercury in the barometer of the pump deſcend to two or three lines of its ſcale, and then let the bladder, which covers the bottle, be punctured.

Immediately the æther will begin to boil, to evaporate with ſurpriſing rapidity, and to be converted into an elaſtic fluid, which, in the winter, will keep the barometer up to eight or ten inches, and, in very hot ſummer weather, to twenty or twenty-five inches.

If a ſmall thermometer be introduced into the bottle containing the æther, the fluid in it deſcends conſiderably during the evaporation, on account of the great quantity of free fire which paſſes, in this experiment, into a ſtate of combination, in order to reduce the æther into vapour.

If the air be permitted to re-enter, the thermometer recovers its ordinary height, viz. about 28 inches. But it is very remarkable that the æther, thus mixed with atmoſpheric air, is not thereby condenſed, but remains as a permanently elaſtic fluid, and forms a particular kind of inflammable air, which I have not hitherto had an opportunity of examining.

This experiment ſucceeds with all evaporable fluids, with ſpirit of wine, and even with water; but, with this difference, that the atmoſphere of ſpirit of wine, which is formed in the receiver of the air-pump, can only elevate the mercury one inch in winter, and four or five in ſummer, and the quantity of vapour is leſs than when æther is employed: conſequently, the abſorption of igneous fluid, and the degree of cold produced will be ſmaller. But it is not from hence leſs certain that, in all theſe experiments, the cold which is obſerved, with different fluids, is always nearly proportioned to the quantity of fluid evaporated.

The above phenomena are leſs ſtriking when, inſtead of putting the fluid into a bottle cloſely covered with a bladder, an open veſſel is uſed; but as this manner of conducting the experiment, gives riſe to particular obſervations, which may afford great light to the ſubject in queſtion, it ſeems neceſſary to give ſome account of it.

Let us ſuppoſe ſpirit of wine to be the fluid, the quantity of it, employed in the experiment, to be but ſmall, and the temperature to be fifteen degrees. As ſoon as the barometer of the pump has deſcended to nineteen lines of its ſcale, the ſpirit of wine will begin to boil, but the ebullition will not continue, as in the former experiment: it will ceaſe, on the contrary, for two reaſons, as ſoon as we diſcontinue the pumping: 1ſt. becauſe the ſpirit of wine, as it evaporates, is tranſmuted into an elaſtic fluid; which, forming a kind of atmoſphere that preſſes on the ſurface of the ſpirit, reſiſts the progreſs of the evaporation; and 2dly. becauſe at the inſtant of ebullition a portion of free fire paſſes into the ſtate of combined fire, in order to conſtitute the elaſtic fluid which is formed, and this circumſtance neceſſarily occaſions a cooling of the whole quantity of ſpirit of wine, the neceſſary effect of which muſt be to retard its ebullition: ſo that ſuppoſing, as we have, that the fluid has, at firſt, begun to boil, the barometer being at nineteen lines, it ſhould not boil, after it has been cooled by the firſt ebullition, till the barometer has deſcended to eighteen lines.

If, after the firſt ebullition of the ſpirit of wine, which has been produced under the receiver, the working of the pump be continued, the ſpirit will not again boil ſo rapidly as at firſt, becauſe no greater quantity of ſpirit will evaporate at each ſtroke of the piſton, than will be neceſſary to replace what has been carried off through the capacity of the piſton; ſo that this ebullition will always be ſtronger in proportion as the body of the pump is larger.

These phenomena reſpecting the evaporation of volatile fluids in vacuo, are ſimilar with the volatile alkali, æther, and ſeveral others: but one very important circumſtance is worthy of obſervation, viz. that all theſe fluids evaporate, principally, from the bottom of the veſſel, or in other words, that it is from thence the bubbles are ſeparated. Theſe bubbles mount up, and burſt upon the ſurface, like thoſe of water when boiling in a kettle. The cauſe of this phenomenon appeared to me, at firſt, to be an immediate conſequence of the enſuing facts, and I reaſoned as follows: in proportion as any fluid evaporates, it is cooled, as has been already ſhewn, and does not recover the temperature of the place in which the operation is performed, till the ſurrounding bodies have reſupplied the quantity of free fire, of which the fluid has been deprived. It neceſſarily follows therefore, that the fluid confined under the receiver of an air-pump muſt be colder than the veſſel which contains it. But the hotteſt parts ſhould be the firſt which evaporate, and thoſe will be the hotteſt which are in contact with the ſides and bottom of the veſſel, as receiving heat from it. In a word, it is at the ſurface that the fluid is cooled, and, on the contrary, it is at its bottom, and by its contact with the containing veſſel, that it recovers the igneous fluid from the ſurrounding bodies: it is conſequently from the bottom that the ebullition muſt proceed. But however plauſible this explanation may appear, I confeſs that ſome experiments have rendered it doubtful, and it is not without great heſitation that I have now related it.

But to reſume, in few words, the whole theory of evaporation in vacuo; it appears certain that the tranſmutation of liquids into elaſtic aëriform fluids is ſubject to two laws, the effects of which are oppoſite to each other. On the one part, the degree of heat, to which they are expoſed, tends to evaporate them; on the other, the preſſure of the atmoſphere reſiſts their evaporation; inſomuch that they are either in an elaſtic or liquid ſtate, according as one or other of theſe powers becomes prevalent. But this theory will be greatly elucidated by the experiments which I am now employed in making, in concert with M. de la Place, an account of which I hope ſoon to be able to lay before the academy.

Before I conclude this eſſay, it may be proper to obviate one objection which the favourers of a different opinion will certainly deem unanſwerable. If, as has been declared, elaſtic or aëriform vapours conſiſt of the matter of fire combined with an evaporable fluid; and if no air or aëriform fluid can be formed without a portion of free fire paſſing to a ſtate of combination; it ought to follow that, in every formation of air, cold ſhould be produced; and it will not fail to be remarked that, when calcareous earth and alkalies are combined with acids, ſo far from cold, a ſenſible degree of heat is often obſervable during the *ſeparation*, or rather during the *formation* of the air. An examination of what happens, on this occaſion, inſtead of weakening, is the ſtrongeſt proof of the truth of, this theory.

It muſt be allowed, that in the combination of acids with alkalies or calcareous earths, we ſometimes obſerve heat. But this circumſtance only proves that a greater portion of the matter of fire is diſengaged in theſe combinations, than is neceſſary to the formation of the fixed air, which then recovers its elaſticity. This aſſertion will be proved, by ſhewing that we can, at pleaſure, increaſe or diminiſh the heat, in proportion to the increaſe or diminution of the quantity of fixed air contained in the alkaline baſis. The following experiments are, in my opinion, indiſputable proofs of the fact.

In five ſeparate bottles were placed as many mixtures, each conſiſting of four drachms of concrete volatile alkali diſſolved in two ounces of diſtilled water. This alkali was nearly ſaturated with fixed air. To the firſt of theſe bottles was added one drachm, to the ſecond, two, to the third, three, and, to the fourth, four drachms of quicklime; the fifth was left without any addition. As ſoon as the lime was mixed with the alkaline ſolution, it attracted, from its ſuperior affinity, the fixed air of the volatile alkali, and was precipitated to the bottom of the veſſel, in the ſtate of chalk or efferveſcent calcareous earth. All theſe liquors being decanted, were placed in as many glaſs bottles, and when they had recovered the ſame degree of temperature, they were ſaturated with nitrous acid, moderately diluted, and the change of temperature occaſioned by the efferveſcence, was obſerved by means of one of M. de Luc's mercurial thermometers. The following is an exact relation of the reſults.

THE volatile alkali, to which no addition had been made, and which had been deprived of no part of its air, ſo far from producing any heat, during its ſaturation with the acid, on the contrary, lowered the thermometer two complete degrees.

THE alkaline ſolution which had been deprived of a portion of its fixed air, by the addition of one drachm of lime, afforded two degrees of heat; that to which two drachms of lime had been added gave three degrees; that with three drachms, four degrees; and, laſtly, by that with four drachms of lime, four degrees and half of heat were produced.

THIS laſt alkaline ſolution, though deprived of more of its fixed air than the others, ſtill contained ſufficient to form a briſk efferveſcence with the nitrous acid, though not nearly ſo much as the ſolution to which no lime had been added.

THESE augmentations of cold or heat would have been more ſenſible, if the alkaline ſolutions had contained a greater quantity of ſalt, but the concrete volatile alkali not being ſoluble, in cold water, in greater proportion than that of one part of ſalt in four of water, it was not poſſible for me to procure a ſolution more ſtrongly ſaturated. In order to complete my experiment therefore, I was obliged to have recourſe to volatile alkali obtained by diſtillation with quicklime; and though this alkali was not ſo far concentrated as it might have been, its combination with diluted ſpirit of nitre produced 27 degrees of heat. For the thermometer, which before ſtood at 16, ſuddenly roſe to 43 degrees, at the inſtant of the combination.

THE phenomena are ſimilar, if fixed alkali be employed. A ſolution of this ſalt, entirely diveſted of fixed air, or in a ſtate of cauſticity, produces, with weak nitrous acid, a degree of heat, nearly equal to that of boiling water, whereas a ſolution of the ſame alkali, ſaturated with fixed air, produces 6 degrees of cold.

It is proved, then, that pure alkaline ſalts, whether fixed or volatile, generally produce heat when combining with nitrous acid; but that the heat diminiſhes in proportion to the greater quantity of fixed air they contain; inſomuch that when they are ſaturated with air, ſome degrees of cold are produced. Fixed air, therefore, in paſſing from a concrete ſtate to that of vapour or elaſtic fluid, carries off with it a part of the matter of fire or igneous fluid, which is naturally diſengaged whenever an alkali is united with an acid; and we may conclude that this igneous fluid enters into the compoſition of the fixed air, as into that of all vapours and elaſtic fluids, of every kind.

Of this aſſertion, give me leave to add a yet ſtronger demonſtration. A very weak ſolution of fixed alkaline ſalt, nearly ſaturated with fixed air, was put into a bottle of ſtrong flint-glaſs, into which a ſmall thermometer was introduced; a quantity of ſpirit of nitre was added, and the bottle immediately ſtopped. The preſſure, effected by the want of communication with the atmoſphere, ſoon abated the effervеſcence, and conſequently heat was produced, and the mercury in the thermometer mounted ſeveral degrees; whereas it would have deſcended if the experiment had been made in the open air. Having unſtopped the bottle, after ſome minutes, and agitated the liquor, the effervеſcence which had been before prevented, and, as it were, ſuffocated, recommenced. In ſome time, the heat, which had been acquired while the bottle was cloſed, was diſſipated, and the mercury of the thermometer ſettled ſomewhat below the degree of temperature of the external air.

From all theſe facts, I conclude, as has been already declared, that all vapour, air, and in general, every elaſtic aëriform fluid, is a combination of the matter of fire with a fluid, or even with any volatile ſolid body; and that volatility is nothing elſe, but the property, which bodies poſſeſs, of being, in ſome manner, diſſolved, of combining with the igneous fluid, and, with it, forming aëriform fluids. Experiments, which I deſign, in future, to publiſh, on this ſubject, will ſerve to elucidate my theory in a more clear and perfect manner.

THE END.

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